STUDIES

FROM THE

Yale Psychological Laboratory

EDITED BY

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1898

VOL. VI.

YALE UNIVERSITY
NEW HAVEN, CONN.

THE NEW ERA PRINTING COMPANY, LANCASTER, PA.

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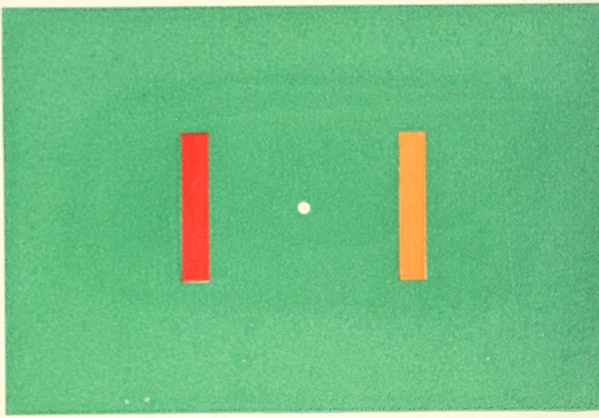
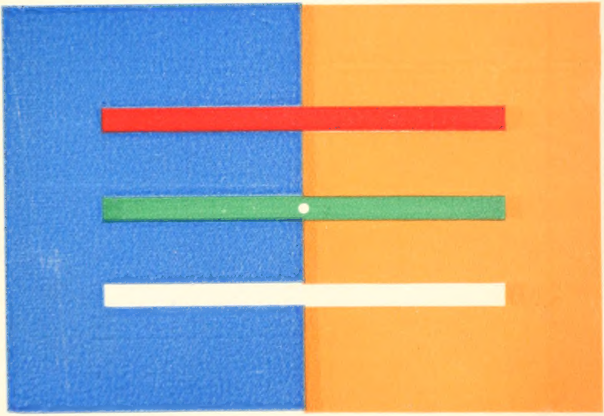




A COLOR ILLUSION,

BY

GEORGE TRUMBULL LADD.

Some time ago my attention was called by Dr. GEORGE T. STEVENS, of New York City, to a color illusion which, in spite of its suggestiveness, has not, so far as I am aware, been hitherto discussed or even noticed.

In FICK's Lehrbuch der Augenheilkunde, p. 50, Leipzig, 1897, there is a colored diagram called an "example of STILLING's charts" for the purpose of testing color blindness. It consists of a pale-green background, in size 36^{mm} by 44^{mm}, which is divided into squares of 1.8^{mm} by lines of white 0.4^{mm} in width; on this background a red letter **E** 21^{mm} by 34^{mm} is constructed out of similar red squares.

It was noticed that, when this figure was observed for a few seconds with a fixed gaze, some or all of the red squares disappeared and were replaced by green squares like those of the background. Practice had the usual result of facilitating the speed and completeness of this illusion; it soon enabled most of those on whom the experiment was tried to get the result in a more or less startling way. I will only add that with me the illusion is invariably connected with a conscious change in fixation of attention, and—if I may be allowed the expression—the internal motor adjustment; and the red squares always turn dark and become a blackish-green for an instant before they disappear and are replaced by the green of the color o the background.

The first suggestion for an explanation of this interesting illusion would connect it with the relations of the retinal images of the two eyes. But such an explanation is at once negatived by the fact that the illusion is obtained equally well, or even better, with one eye. It is thus not an affair peculiar to binocular vision. Neither is it a phenomenon of blurring due to minute rapid movements of the eyes, or to relaxation of the muscle of accommodation. For, although in certain experiments to be described subsequently, the color of the background does frequently seem to throw a film of its own color over the letter, square or strip, which it surrounds, in the original figure as taken from FICK there is no blurring of the red squares, and in all the other experiments the white

lines and outlines of the squares and the color of the background remain quite distinct and sharp.

Further experiments have been devised to investigate this illusion. Although no final conclusion has been as yet established, enough has been done to show that the phenomenon is more complicated than was at first supposed. A brief description of these experiments will now follow.

In the first place, within certain limits, not as yet fixed, changes in the size of the usual objects with which the experiment is conducted do not essentially alter the result. For example, if the diagram in FICK be looked at through a concave lens the diminished letter **E** will behave in the same way; although with me it is more difficult to get the illusion, because, as I think, both of the increased clearness of the images and also of the increased tension of the attention and the eyes' adjustment. On the other hand, as all our experiments show, much larger objects, when viewed from a sufficient distance behave in the same way. The exact limits of size of the retinal images within which this phenomenon is possible have not been calculated, but considerable variation in size is known not to be incompatible with its occurrence.

If, now, in order to test the importance of the white lines which divide, in the FICK diagram, both background (or "substituting" color) and letter (or "disappearing" color) into squares, we have a strip composed of red squares divided by white lines against an undivided background of lighter or darker green, we get the following result: the red turns dark green or black; the white lines remain; and the squares of red become patches of blackish color separated by white outlines on an unchanged color-background. If the background be changed from green to violet or blue the results are substantially the same. But the same change in color takes place with a solid red letter on a background of various colors; and with a strip of red or orange, not limited in any way by white outlines, on variously colored backgrounds. Indeed, in a number of instances, as I shall soon say, the background substitutes its color and the illusion takes place almost or quite equally well without either being itself divided or having the disappearing color divided into squares. The illusion, then, is not dependent upon the lines that divide the diagram into squares, although such a division seems to have some influence on the speed and completeness of the result.

A series of experiments was then devised by Dr. Scripture to test the effect of varying the color of the background, or substituting color, while retaining the same red as the disappearing color, without, however, dividing either background or strip into squares.

A complete set of the MILTON BRADLEY colored papers was obtained;

in the following experiments the colors will be designated according to the system adopted by the manufacturers. Sheets 20^{cm} by 30^{cm} were cut out for backgrounds; small strips 1^{cm} by 20^{cm} were cut ready to lay on the backgrounds. In making the experiment a background of a desired color was fastened to a board by tacks and a strip was fixed on it by a pin; the whole was then observed at a distance of about 3^{m}. It afterwards appeared that the most convenient method of preparing these figures was to place several strips of different colors on a given background; such a figure with two strips and a white fixation point is shown in the colored plate.

The results with Dr. Scripture and myself differed somewhat; but the interesting thing about these differences seems to be that they remained fairly constant of the same order. That is to say, where I got the illusion of substitution without great difficulty, my colleague got it more readily; and when I got it not at all, he could obtain it only with increased difficulty, or in certain cases (at least on the earlier trials) not at all. This suggests that the detailed differences were due to a constant factor of difference in the two observers.

The results obtained by viewing a standard red strip at a convenient distance, and with the proper fixation of the eye, while the color of the background is changed, may be divided into two general classes. With certain backgrounds the illusion of disappearance and substitution takes place with no great difficulty but with surprising ease and suddenness, after a little practice has given the requisite knowledge of how to experiment. With certain other backgrounds the substitution takes place only with increased difficulty or not at all, with me as a rule not at all. To the first class of backgrounds belong the two greens used, namely, standard green and bluish-green, violet, blue and black. To the second class belong the backgrounds, yellow, orange, light gray, white, light blue, light green and a light reddish violet. That is to say, this standard red strip, if viewed with a proper amount and kind of fixation on a background of two kinds of green, or of dark violet, dark blue, or black, will itself darken, disappear, and be placed for a longer or shorter interval by the color of the background. But the same strip, if placed upon a background of yellow, orange, gray, white, or light blue, light green, light violet, will maintain its place, although growing darker; or if it disappears at all, does so with great difficulty and only for an instant. As I have already said, I could not myself get the illusion in these cases at all.

Suppose now that a strip of orange be viewed upon the same backgrounds as in the foregoing experiments, then somewhat similar phenom-

ena result, but with interesting differences. With the backgrounds of green, dark violet, dark blue or black I get the illusion; but only by persistent trying, and for a briefest instant of time. On the backgrounds of yellow, gray, white, light blue, light green, violet of tint No. 1 Dr. Scripture obtains the illusion only with great difficulty, if at all, and then for a brief instant of time. But on the other class of backgrounds the illusion is rather more easily obtained by him with the orange than with the red strip. With me, however, on both classes of backgrounds, the red strip and the orange strip behave in markedly different ways. Whereas, in the case of the red strip the color darkens—and this whether substitution ultimately takes place or not, and whatever the color of the background—in the case of the orange strip, the color grows lighter, the color of the background seems to encroach from both sides on the color of the strip, until (where—as in all cases of the second class of backgrounds—the illusion does not become perfect) the orange strip becomes a narrow line of sunlight on a unicolored background.

If now the orange and the red strips be hung not far apart on the same background of color, each of them behaves, for both observers, in the same way as when viewed apart on a similarly colored background. And if orange and red strips be placed side by side, with one half over a background of the first class (see the colored plate) and the other half over a background of the second class, each half of each strip will seem to follow, for each observer, the course similar to that already described in the separate experiments.

This color phenomenon is probably a general one. It was observed by most persons at a meeting of the American Psychological Association at which these figures were exhibited. It was quite evident at the first trial to a girl fourteen years of age who was asked to observe the strips. I am not yet able to satisfy myself as to the most probable explanation of this somewhat startling color illusion. In the well-known case of a dark spot on a colored background B[illegible][1] has suggested that with long fixation the retina becomes less sensitive on the parts on which the colored rays fall, and more sensitive on the part corresponding to the dark spot, and that this latter portion, being sensitive to the colored light arriving after general dispersion in the eye, finally gives as intense a color sensation as the other portion. Any one, however, who has seen the red stripe suddenly turn to a bright green knows that the intensity of the green is far beyond any light that might arrive through dispersion.

Dr. Scripture has suggested that the fatigue of the eye for the color of

[1] [illegible] Inhibition, Skand. Arch. f. Physiol., 18[illegible]; V 1; (reviewed in Z[illegible], [illegible]; VII 411).

the disappearing strip or letter creates a temporary blind-spot or succession of blind-spots, which is then filled in by the color of the background as the permanent blind-spot of the eye is constantly filled in by the surrounding colors in all our normal vision. This suggestion explains some of the phenomena which I have been describing. It explains the dependence of the result on the character of the fixation, the customary preliminary darkening of the disappearing color, and the character of the substitute color, when substitution takes place. But I am not able to reconcile this suggestion with the fact that the illusion takes place only with such great difficulty, or not at all, when the disappearing color is dark and the background is light. Would not one expect rather the opposite result, namely, that the darker background would more speedily and completely fill in the eye fatigued by the lighter color. Moreover, I cannot see why the orange strip under the principle of fatigue, should grow brighter and of a lighter shade, as it certainly does for my eye ; and, in fine, why in the case of the second class of backgrounds, I am quite unable to fatigue my eye for either orange or red so as to obtain the illusion by substitution of the color of these backgrounds.

Finally, as far as I can determine in my own case, and by questioning several others with whom the experiment has been tried, the illusion is somehow dependent upon the rhythm of attention, and, in a limited way, it is under the control of will exerted through some obscure modification of the point and manner of regard. But whatever the *prima facie* explanations may be, the illusion seems to me unusually interesting and complicated, and in its suggestiveness quite worthy of further investigation.

RESEARCHES IN CROSS-EDUCATION,

BY

WALTER W. DAVIS.

I. HISTORICAL.

The term "cross-education" has been used[1] to express the fact that the effects of practice on one side of the body are transferred to the unpracticed side. The fact seems to have been first recorded by H. F. WEBER. In a communication to FECHNER,[2] he reports an observation made on his son. The boy had been taught to write entirely in one system of penmanship, a system that employs a free-arm muscular movement. At the age of thirteen he was able to write reversely with the left hand —in so-called mirror-writing—without having practiced such writing in the least, although the letters were not so regularly made as those made with the right hand. The reverse writing, when viewed in a mirror, or when looked at through the paper as it was held to the light, appeared very similar to the boy's ordinary hand-writing. Hence WEBER concluded that by the training of the right hand in certain methods of penmanship the left hand is also trained, unconsciously, to perform symmetrical movements. He noticed also that others trained by different methods, or by several methods, failed in the test.

FECHNER reached a similar conclusion from an experience of his own. In the course of a series of observations in which he wrote the figure *9* many times, left-handed, he noticed that when he took the pen in his right hand, he would unconsciously write the figure reversed with a movement that was symmetrical to that made with the left hand. A certain method of writing had become so "impressed upon his mind" that it became natural to write reversely with the hand not used.

[1] SCRIPTURE, SMITH and BROWN, *On the education of muscular control and power*, Stud. Yale Psych. Lab., 1894 II 115.

[2] FECHNER, *Beobachtungen, welche zu beweisen scheinen, dass durch die Uebung der Glieder der einen Seite die der andern zugleich mitgeübt werden*, Ber. d. kgl.-säch. Ges. d. Wiss., math.-phys. Cl., 1858 X 70.

The experiments of VOLKMANN,[1] on the influence of practice upon the power for perceiving small distances, have a bearing on cross-education. By touching a WEBER's compass to the skin on various parts of the body, he obtained records of the ability of the several members to distinguish the two points of the compass at the smallest perceptible spread. For example, the left arm on the dorsal side could distinguish the two points at 10.5 Parisian lines (23.6^{mm}); the right arm at 11.5 lines (26.4^{mm}). At the end of the practice which was continued for several weeks with the left arm alone, the records were: left, 5 lines (11.2^{mm}), right, 7 lines (15.7^{mm}). While the acuteness of sense on the left side was increased through local practice, it was also increased on the right side in portions symmetrical with the parts practiced.

To determine whether other than symmetrical parts are thus trained, VOLKMANN tested the points of the fingers of both hands and also the left arm. By practice of one of the fingers of the left hand, he found an increase in ability in all the fingers, but none in the arm. That is, in the education of certain parts, those parts symmetrical and closely related are educated also.

More recently SCRIPTURE[2] has made some experiments in muscular control and muscular power which prove quite definitely that practice of one arm in steadiness and strength reacts on the other arm as well. DR. W. G. ANDERSON, Associate Director of the Yale Gymnasium, experimenting with the spring dynamometer, has reached practically the same conclusion. BRYAN,[3] in testing the tapping ability of children of different ages, concludes that the right hand does not outgrow the left, but that, at certain ages, the left even gains on the right.

The following experiments were carried on, during the academical year of 1898–99, for the purpose of establishing more definitely the fact of cross-education or transference of practice and, if possible, of finding the causes of such transference.

II. RAPIDITY OF VOLUNTARY EFFORT.

The present investigation was begun by experiments in the rapidity of tapping on a telegraph key. The movement in tapping involved only a small amount of muscular strength. The weight of the finger was sufficient

[1] VOLKMANN, *Ueber den Einfluss der Uebung auf das Erkennen raümlicher Distanzen*, Ber. d. kgl.-sächs. Ges. d. Wiss., math.-phys. Cl., 1858 X 38.

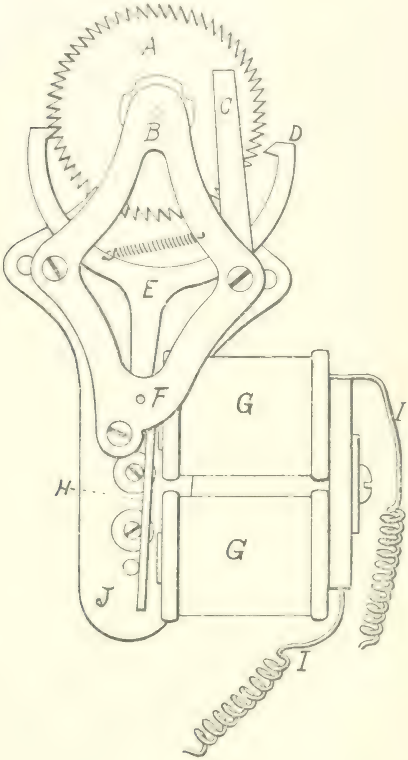
[2] SCRIPTURE, SMITH AND BROWN, *On the education of muscular control and power*, Stud. Yale Psych. Lab., 1884 II 114.

[3] BRYAN, *On the development of voluntary motor ability*, Amer. Jour. Psych., 1892–93 V 201.

to press down the button of the telegraph key, so that the test was one of motor ability with the factor of muscular power seemingly almost eliminated.

Apparatus.

The tapping was done by the subject in a quiet room, while the results were recorded in an adjoining room. The number of taps in a given



[illegible]

time was recorded on a tap counter which was connected by electric wires with the key. It consisted of clock-work with hour and minute

hands. At the back (Fig. 1) there is a toothed wheel worked by an electro-magnet (*G*) and an armature (*H*). When a current passes through the magnet, the armature is attracted; when it ceases, the armature is drawn back again by a wire spring. The upper end of the lever (*E*), of which the armature is a part, divides into two arms (*D*), each of which, one on each side, may press successively into a toothed wheel (*A*) in such a way that at each make and break of the electric current the wheel is driven forward one cog. This movement arises from the shape of the ends of the arms and of the teeth of the escapement wheel. A friction spring (*C*) hinders any backward movement. This counter has been in use in the laboratory for several years. Its special merit lies in the fact that it will count the most rapid taps that a person can possibly make. The number of taps is recorded on the face of the counter. A key within easy reach makes it possible, while the subject is tapping in the quiet room, for the observer to let the current pass or not, as he chooses.

In the quiet room, three keys were so arranged that the subject could, without changing his general position, tap with either hand or either foot. One key served for the hands and was clamped to a board which was held in the lap. With the subject seated in readiness for tapping, the board was supported by the arms of the chair and the elbows rested easily on the board, the hand just reaching the key. The tapping was done with the index finger, the subject being instructed to keep the other fingers, the wrist and the forearm on the board. This position allowed a rapid, easy, isolated movement without the use of clamps or weights that might distract the attention.

For the feet a board was so constructed that two keys, four inches apart, could be set into the top, their ends touching at one edge and leaving the buttons only a little more than flush with the surface of the board. The tapping was restricted to the great toe, all movement being confined as much as possible to this member. The chair remained always in the same position and the board could be moved on the floor, forward or backward, to suit the convenience of each subject. When a certain position was adopted for any subject, the conditions remained constant for him through the entire series of experiments.

For the purpose of communication the two rooms were connected by a system of signals, by which the observer could direct the subject to start or stop. In all cases the number of taps in five seconds was recorded. With all in readiness, the observer, with watch at hand, signaled the subject to start. At three seconds after the start the switch was closed and the counter began to record. After five seconds the switch key was opened and the subject signaled to stop.

Method of experiment.

In this series of experiments, the results of which are shown in Table I., tests of tapping ability were made on six subjects. Of these, A. is an instructor, and B., C., D. and F. are students in Yale University. E. is the steward of the Psychological Laboratory. Initial tests were taken of the right and left index fingers, and of the right and left great toes. Then for periods varying from ten to twenty days the *right great toe* was practiced daily, or nearly so. At the end of the practice period final records were taken of each member. The initial and final tests were taken in the following order: for subject A. *LF RF LH RH RH LH RF LF*; for B. *LF RF LH RH LF RF LH RH*; for C., D., E. and F., *LF RF RH LH LH RH RF LF* (*RH*—right hand, *LH*—left hand, *RF*—right foot, *LF*—left foot). In this way the liability to differences due to fatigue was guarded against. In each initial and each final test, averages were taken of the two records made by each member. A specimen record is inserted, the first one taken.

Subject A.—Initial test.

	LF	*RF*	*LH*	*RH*	*RH*	*LH*	*RF*	*LF*
	13	17	24	26	28	24	19	11
Average	12	18	24	27				

The initial and final tests were taken at the same time of the day, and, as nearly as possible, under similar conditions. LOMBARD[1] and DRESSLAR[2] have pointed out the differences in voluntary strength and rapidity due to varying conditions. An effort was made to equalize them for all records.

Results.

The results obtained are shown in Table I., which gives the numbers of taps in five seconds for each member at both initial and final tests, also the gain in the number of taps, and the gain as a percentage of the initial number.

The effects of practice are not uniform. Not only is there a great difference between the gains made by different individuals, but also between the gains of different members of the same individual. Still, certain fundamental results are clearly shown. In the first four subjects, whose initial and final records are given in Table I., a marked increase in the rapidity of tapping ability has been developed; and this increase mani-

[1] LOMBARD, *Some of the influences which affect the power of voluntary muscular contraction*, Jour. Physiol., 1892 XIII 2.

[2] DRESSLAR, *Some influences affecting the rate of voluntary movement*, Amer. Jour. Psych., 1892 IV 514.

fests itself not only in the right foot, which alone was practiced, but in all the other members as well. However, the greatest average gain is

TABLE I.

Number of taps in 5 seconds.

Subject.		Initial test.	Final test.	Gain.	Relative gain.
A.	*LF*	12	17	5	0.41
	RF	18	20	2	0.11
	LH	24	28	4	0.17
	RH	27	32	5	0.18
B.	*LF*	15	17	2	0.13
	RF	14	21	7	0.50
	LH	22	36	14	0.64
	RH	22	34	12	0.54
C.	*LF*	20	26	6	0.30
	RF	21	28	7	0.33
	LH	32	36	4	0.13
	RH	37	42	5	0.14
D.	*LF*	17	25	8	0.47
	RF	20	28	8	0.40
	LH	26	34	8	0.31
	RH	27	29	2	0.07
E.	*LF*	16	22	6	0.38
	RF	21	20	—1	—0.04
	LH	22	24	2	0.09
	RH	24	22	—2	—0.08
F.	*LF*	16	17	1	0.06
	RF	21	19	—2	—0.09
	LH	30	24	—6	—0.20
	RH	33	28	—5	—0.15
Average.	*LF*	16	21	5	0.31
	RF	19	23	4	0.21
	LH	26	30	4	0.15
	RH	28	31	3	0.08
Average: A, B, C, D.	*LF*	16	21	5	0.31
	RF	18	24	6	0.33
	LH	26	34	8	0.31
	RH	28	34	6	0.21

made by the member practiced. The gain by *RF* is slightly greater than that of *LF* and *LH*, and considerably greater than that of *RH*.

In neither E. nor F. is there a gain in the member practiced, but rather a slight loss. In only one member not practiced, *LF* of subject E., is there a marked gain, while in *RF*, *LH*, and *RH* of subject F., are there marked losses.

In the second average the records of subjects E. and F. were not used, the object being to give the comparative average results of practiced and unpracticed members in subjects who made a gain in the member practiced.

From certain observations taken during experimentation, as well as from remarks and suggestions made by the subjects themselves, these facts appear to be satisfactorily explained. Subject E. is a colored man, the steward of the laboratory, thirty-four years of age, of a phlegmatic temperament, steady-going rather than quick and active. The two facts of age and temperament are suggested as the reason for his not responding more quickly to practice. In the gymnasium it is illustrated every day that after a certain age the boy loses in a great degree the ability to learn new tricks. Moreover boys differ individually in the ability to learn. This difference is not due wholly to lack of strength but to temperament, will-power and previous training as well. Possibly, with longer practice, good results might have been secured from E. Subject A. was of the same age as E., but the practice in his case was continued twenty days and in the case of E. only ten days; the two subjects differ much in temperament, A. being nervous rather than sluggish.

Table II.

[illegible]

Subjects.	Age.	Average gain of the four members.	Relative gain.	Days of practice.	Relative gain per day.
A.	34	4	0.20	20	0.01
B.	29	9	0.44	14	0.031
C.	25	6	0.22	10	0.022
D.	27	6	0.27	10	0.027

Age seems to play an important part in the results obtained from practice. Table II. gives the ages of the subjects in connection with the average gain they make for both hands and both feet, the length of practice, and the gain per cent. for each day of practice. To obtain a fair increase in the rapidity of tapping, the practice of A., who is 34 years old, had to be continued for 20 days, and then the gain is less than that of B., C. and D., where the practice was continued for 14, 10 and 10 days respectively, and the ages were 29, 25 and 27 years.

An interesting principle was exemplified in the failure of F. to make a gain in rapidity by practice. F. is an academical senior, aged 23, the youngest of the six subjects, and of a nervous motor temperament. He is a trained gymnast of more than moderate skill. One would judge that conditions here were favorable for a rapid gain in tapping ability. Some of his remarks, however, after the daily practice, throw light on the difficulty. "If I try to hurry too much my foot stops almost altogether." "At times I am obliged to put forth my entire will power in order to tap at all." "I feel fatigued all over"; that is, fatigue did not confine itself

to the muscles involved but was general. It would seem that, in this subject, temperament and training had combined to form conditions unfavorable to rapidity of tapping. Naturally of a quick, nervous, active disposition, his training as a gymnast had emphasized these qualities. The proper execution of difficult gymnastic tricks requires a great exertion of strength and also its quick concentration at a particular moment in time. The subject had become so accustomed to sending down strong impulses to action to large muscles, that it was difficult for him to send proper impulses for the action of small muscles like those of the fingers and toes. Subject F.'s great store of energy is illustrated in his long practice records. It seemed impossible to tire him out. In one experiment he made 900 taps without a slowing in rapidity of tap-time. He concentrated more attention at one point than was favorable for rapidity of movement. So great an amount of nervous energy was sent to this point that the delicate muscles could not properly dispose of it for action, and the result was a slowing of movement.

On the whole, the subjects were a group not favorable for rapid improvement from practice. Of an average age of 28 years, they had gotten beyond the point in physical development where either muscular or nervous changes would take place rapidly. With younger subjects larger and more rapid gains might reasonably be expected. Nevertheless, the principle of cross-education appears most plainly and decisively.

Since obtaining the above results, I have received a letter from Herr Oscar Raif, Professor of Music in the Berlin Hochschule, who has done some experimenting similar to mine. I quote a portion of his letter, inserting the number of beats per minute in [ ].

"In the spring of '98, I made an experiment with twenty of my pupils. I began by taking the average speed of each hand with the metronome. The average of the right hand was ♩ = 116 (= four times 116 in the minute) [464 beats] and for the left hand, 112 [448 beats]. I gave them exercises for the right hand only (finger exercises, scales and broken accords) to develop rapidity. After one week the average of the right hand was 120 [480], after two weeks, 126 [504], three weeks, 132 [528], etc. After two months the right hand yielded 176 [704]. Then I had them try the left hand which averaged 152 [608], whereas in November the average was only 112 [448]. In two months' time, absolutely without practice, the left hand had risen from 112 [448] to 152 [608]. A few of my pupils had some difficulty in playing the scales in parallel motion, but were able to play them in contrary motion.

"The tenor of my work is that in piano playing the chief requirement is *not* that each single finger should move rapidly but that each movement

should come at exactly the right time, and we do not work only to get limber fingers but more than that to get perfect control over each finger. The source of what in German is called 'Fingerfertigkeit' is the center of our nervous system, the brain."

Further explanation of the application of these principles in musical training is promised in a work on "Fingerfertigkeit" by Professor RAIF. The fact that Herr RAIF's pupils could play the scales in *contrary* more easily than in *parallel* motion, deserves notice as coinciding with the observations made by WEBER and FECHNER in regard to mirror-writing.

Table III. gives the relative rapidities of hands and feet, both before and after practice, and their average percentage of gain. The ratio of tapping ability is smaller at the end of the practice than at the beginning; the relative gain of the feet being 5.5% greater than that of the hands.

TABLE III.

Comparative rapidity of hands and feet.

		Initial.	Final.	Relative gain.
A.	Feet.	15	18	0.24
	Hands.	25	30	0.16
	Ratio.	1:1.7	1:1.6	
B.	Feet.	14	18	0.31
	Hands.	22	35	0.58
	Ratio.	1:1.5	1:1.8	
C.	Feet.	20	27	0.32
	Hands.	34	38	0.12
	Ratio.	1:1.6	1:1.4	
D.	Feet.	19	26	0.39
	Hands.	27	31	0.17
	Ratio.	1:1.4	1:1.2	
Total Average.	Feet.	17½	22½	0.31
	Hands.	27½	33½	0.26
	Ratio.	1:1.6	1:1.5	

In the case of D. two facts are noticeable from an examination of the table: (1) lower ratios between feet and hands both before and after practice; and (2) a greater percentage of gain in the tapping ability of the feet in comparison with the other subjects. During D.'s practice it was observed that he had an almost independent use of his great toe. He could flex or extend it, with very little accompanying movement of the remaining toes. D. is a Japanese student and while in Japan wears the ordinary clog shoe which allows free movement of the great toe. A heavy string extending up for an inch from the middle forward part of the sole divides into two parts which pass back, one on either side of the foot. When the shoe is adjusted to the foot, the string before its division

is grasped between the first and second toes, and the shoe is held in place by this means. The sock also is made in the form of a mitten, the great toe being in a separate compartment from the others. The sock is the only covering of the foot above and to the sides. Hence two prime conditions for development are here present, exercise and room for growth. These facts probably account for the smaller ratio between the rapidity of toes and fingers in the case of D. They also emphasize the fact already pointed out [1] that the feet of Americans are losing, as a result of tight, ill-formed shoes, those powers natural to them. The reason why the feet in the case of D. should make a greater percentage of gain than in the other subjects, although not so clear, is probably due to the same cause.

Influences affecting the rapidity of tapping.

In the daily practice the right great toe tapped until fatigued. There was, however, no attempt to reduce the muscles to a state of extreme fatigue, since the increase in tapping ability at the final test was the prime object. The records were made in a manner similar to those at the initial and final tests. The signal to start was given; after three seconds the switch was closed and a record was obtained for five seconds. The result was noted while the subject continued to tap without stopping, and after ten seconds another record was taken. The subject and the observer continued in this manner, the record being taken at every third period of five seconds, until the subject stopped tapping.

A study of these records and of the remarks dropped by the subjects led to several important observations.

(*a*) There are perceptible variations in the rapidity of the tap-time on any given occasion of practice. One subject says: "At times it is very easy to tap rapidly; then it becomes difficult; then easy again." Another subject noticed what he termed "waves," that is, short periods of rapidity followed by a slowing of the tap-time. These waves of rhythm have been noticed by NOYES [2] in the knee-jerk; and also by LOMBARD [3] in his work with the ergograph. They seem to be wholly beyond the control of the will.

This phenomenon is shown in Fig. 2 which gives the curve for a record by A., the twelfth in his series of practice records. X indicates the serial number of the record, and Y the number of taps in five sec-

[1] ELLIS, *The human foot*, Wood's Medical and Surgical Monographs, April 1890.

[2] NOYES, *On certain peculiarities of the knee-jerk in sleep*, Amer. Jour. Psych., 1892 IV 343.

[3] LOMBARD, *Alterations in the strength which occur during fatiguing voluntary muscular work*, Jour. Physiol., 1893 XIV 98.

onds. The continual fluctuation between gain and loss in energy is quite striking.

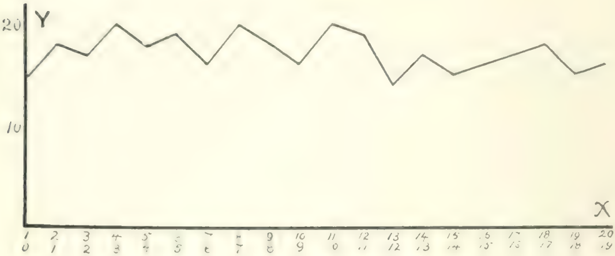


FIG. 2.

X, upper line, serial number of record.
X, lower line, successive steps in the subject's practice.
Y, number of taps in 5 seconds.

(*b*) States of feeling do not appear to have much effect on the rapidity of tapping. Often the subject would remark that he did not feel like making a good record and when the record was taken it would prove excellent. The reverse is true also. One of the subjects remarked before a certain practice that he had partaken of a fine punch the night before, had had a good night's rest, felt in excellent spirits and expected to make an unusual record. After the experiment the result was compared with the previous ones and the taps were found to be slower than any in the preceding three days. There were many other similar, though less notable, instances of the deceptiveness of the subject's judgment of his own condition.

(*c*) On the other hand, the physical condition of the hand or foot as judged by sensations had a noticeable effect on the tapping ability. On one very cold morning the subjects, without exception, made poor records; most of them had complained of cold feet, due to the exposure of the bare foot in the cool air.

TABLE IV.

[illegible]

Day	1	2	3	4	5	6	7	8	9
Record during first 5 seconds	[illegible]	15	14	[illegible]	17	20	[illegible]	17	23
Record during second 5 seconds	24	[illegible]	18	24	24	28	28	22	22

(*d*) Tapping could be done faster and more easily after a few taps had been made, or as C. expressed it, after the muscles had been "warmed

up." Table IV. gives the first two records of each of the practice experiments for D.

The results show that either the muscular or the nervous system or both do not do their best work at the start.

(*e*) Pain and fatigue, when they were noticed at all as the result of continued tapping, were generally located in or near the muscles employed in the movement. Rarely indeed was fatigue spoken of as being general.

(*f*) The length of time during which the subject kept up his practice was a good indication of his store of energy. There was a tendency to longer practice-records on Mondays and on any day preceded by a day of rest from tapping. Apparently the muscular or nervous energy expended in the performance of one day's task was not fully restored in one day, the tissues requiring a day's rest to regain their normal capacity for work.

(*g*) Tapping became much easier after a few days of practice ; the apparent reason was that it did not require so much attention. This points to the probability that lower, automatic centers were being developed for the foot in the specific movement of tapping, leaving less responsibility for the act upon the higher centers of consciousness and attention.

(*h*) Subject A. experienced sensations of pain in his unused left great toe similar to those in the right which was being fatigued by the tapping. He has noticed the same fact in connection with "writer's cramp" which has troubled him at various times ; the numb sensation of the right middle finger is sometimes transferred to the left middle finger for periods varying from a few minutes to hours. He does not use the left hand for writing.

The loss of the effects of practice.

To determine if the effects of practice are retained for a long period of time, A.'s record was obtained six weeks after his final test. Table V. shows the results compared with his initial and final tests.

TABLE V.

Effect of a long interval.

Subject.		Initial.	Final.	After six weeks.
A	*LF*	12	17	18
	RF	18	19	16
	LH	24	28	24
	RH	27	32	30
Average		20	24	22

There was a general loss after the interval. This loss was more marked in the hands than in the feet; the average loss for the hands was 3; for the feet, 1.

III. STRENGTH OF VOLUNTARY EFFORT.

It has already been proved that the development in the strength of one arm is accompanied by an increase in the exertion that can be put forth by the other.[1] Is this increased power due to increased nervous activity or to increased muscular tissue?

The following experiments were undertaken primarily to determine if an increase in the size of one arm would result from the exercise of the other in muscular strength and endurance. From previous experiments, referred to in Sec. I., an increase of strength was looked for in both arms. It was not believed that a perceptible increase in the girth of the left arm would result after so short a period of training.

Method of experimenting.

The experiments were made in the following way: six subjects were chosen, definite girth measurements of both arms were taken, and the number of times ascertained that each arm could raise a weight of 2¼ kilos. (5 pounds).

At the initial test the subject's clothing was removed from the upper part of his body. His weight was then taken and his strength of forearm, or grip, measured by the usual oval spring dynamometer. The following measurements were then made by Dr. J. W. Seaver, Associate Director of the Yale Gymnasium: right and left upper arm both flexed and extended; right and left forearm with and without the hand clenched. These measurements were taken at the largest circumferences of the arm above and below the elbow. The weight (a 2½ kilo. dumbbell) was then given to the subject, who was instructed to lift it from a position where the arm hangs extended downward and the weight is supported from the shoulder, to one where the arm is flexed and the weight close to the shoulder. In this movement the elbow remains stationary. Hence, to accomplish this act, the biceps is employed almost wholly, though the muscles of the forearm are also used to a lesser extent in gripping the dumbbell. This gripping was intensified toward the end of the test, when the subject became fatigued.

A metronome was not used, but each subject was allowed to fall into his own rhythm of movement, which varied according to the length of the arm

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[1] Scripture, Smith and Brown, *On the education of muscular control and power*, Stud. Yale Psych. Lab., 1894 II 114

and the man's temperament. The only instruction he received was not to allow the weight to stop at its highest and lowest points. At the final test it was observed that the rate of motion was unconsciously faster than at the initial test. This was probably due to an increase of energy stored up during the practice.

Fatigue was not carried to an extreme, because such a test would put the arm and the physical system in general into such a condition that good results could not be expected from the succeeding short practice. When the right arm was fatigued, a rest of five minutes was given, after which the left arm went through the same exercise.

The subject then entered upon a practice extending from two to four weeks ; this consisted in simple flexions of the *right arm* with the weight. The subjects were instructed not to tire the arm but to exercise it frequently and lightly, rather than heavily and at long intervals.

At the final test the same data were obtained in the same way and under the same conditions as at the initial test. Additional data were also obtained, to be spoken of later. The arms were examined by pressure to detect any changes in condition that might have been occasioned by practice.

Characteristics of the subjects.

As the results indicated that the age, physical condition and mode of life of each subject should be taken into consideration, the following data were collected :

G. Age, 28. Health, fair. Temperament, nervous. Muscles, soft, undeveloped. Exercise, light gymnasium.

H. Age, 26. Health, good. Temperament, phlegmatic. Muscles, soft. Exercise, light.

B. Age, 29. Health, good. Temperament, nervous. Muscles, soft. Exercise, none. Left-handed in most actions.

I. Age 26. Health, good. Temperament, nervous. Muscles, firm, well developed Exercise, regular, in gymnasium.

J. Age, 26. Health, excellent. Temperament, motor. Muscles, well developed, but soft. Exercise, none.

K. Age, 24. Health, not good, over-worked. Temperament, nervous. Muscles, soft and very poorly developed.

The subjects were all in a muscular condition favorable to increase in girth measurements, but their average age of 26 years was probably unfavorable for such an increase. They were all members of the Graduate School of Yale University. Those who were taking any physical exercise, were instructed to continue it throughout the practice.

Increase in the dimensions of the arms.

Table VI shows the girth measurements taken at the initial and final tests and the increase due to the practice. The subject's age and weight are given as a means of control. The measurements are in millimeters.

TABLE VI.

Increase in arm girth.

Subject. Age.		Biceps. Right. C.	Biceps. Right. R.	Biceps. Left. C.	Biceps. Left. R.	Forearm. Right. C.	Forearm. Right. R.	Forearm. Left. C.	Forearm. Left. R.	Wt. in kilos.
G. 28	Initial.	283	228	274	220	259	255	256	246	50.3
	Final.	288	232	269	226	260	255	254	246	50.8
	Gain.	5	4	—5	0	1	0	—2	0	0.5
H. 26	Initial.	270	238	265	237	252	247	250	248	66.6
	Final.	283	246	271	241	265	257	260	252	67.2
	Gain.	13	8	6	4	13	10	10	4	0.6
B. 29	Initial.	287	249	285	252	246	237	260	252	75.7
	Final.	293	255	296	256	259	249	265	257	75.1
	Gain.	6	6	11	4	13	12	5	5	—0.6
I. 26	Initial.	290	275	317	275	283	275	281	272	71.6
	Final.	298	281	320	277	283	277	277	270	71.6
	Gain.	8	6	3	2	0	2	—4	2	0
J. 26	Initial.	310	265	302	260	270	262	265	255	69.3
	Final.	312	264	302	262	273	265	266	260	69.3
	Gain.	2	—1	0	2	3	3	1	5	0
K. 24	Initial.	276	235	260	220	252	240	243	232	56.1
	Final.	280	239	262	230	251	240	246	235	56.1
	Gain.	4	4	2	10	—1	0	3	3	0
	Av. gain.	6.3	4.5	2.8	3.6	4.8	4.1	2.1	2.5	0.1

C = Contracted. R = Relaxed.

The results showed that the practice had effected an increase in the girth measurements. For example, G.'s initial measurement for the right biceps contracted was 283, relaxed it was 228. His final measurements for the right biceps were, contracted 288, relaxed 232. The gain in the biceps contracted was 5^{mm} and relaxed 4^{mm}, as the result of his two weeks' practice.

There was no marked gain in the weight of any of the subjects that might account for the increase in girth. The average gain was 0.1 kilo, which is insignificant, while B., who lost weight, made as large gains in girth as any.

The gains in girth measurements were greatest in this order; right biceps, right forearm, left biceps, left forearm. The right arm gained by direct practice but though the right increased twice as much as the

left, still the left made marked gains, and there seems to be no doubt that there had been a transference of the effects of practice on the one side, to the unpracticed other side. Among the girths of right biceps there is only one loss recorded. This is for J., who made only small gains in all of his girths. For left biceps there is one loss recorded, that of G. This, as well as other irregularities in the case of G., to be mentioned later, may be explained from the fact that he misunderstood the instructions given, and during his practice period, stopped his usual exercise. For the right forearm one small loss is recorded, and for left forearm three losses.

The results, though exhibiting variations and exceptions, show very clearly that exercise producing a gain in girth of one arm causes a similar though smaller gain in the other.

Symmetrical development.

Here, it would seem, is a provision by nature to prevent a one-sided development. If the right side of the body received all the benefit of its excess of exercise over the left, it would tend to outgrow it in much greater proportion than is actually the case.

There is really very little difference between the sizes of the right and left arms. This is especially true if the measurements are taken with the muscles relaxed. There is a greater difference when contracted, as if a stronger stimulus to action could be sent to the right than to the left. As a proof of this point the measurements of one hundred Yale Freshmen were averaged, and the following results obtained.

Right biceps, contracted,	296.04^{mm}
Left " "	282.57
Difference,	13.47
Right biceps, relaxed,	248.95
Left " "	242.41
Difference,	6.54

The difference in girth between the right and left biceps, contracted, is 13.47, but when relaxed, the difference is only 6.54. If we subtract the girths with muscles relaxed, from the girths with muscles contracted, we find a difference in the right arm of 47.09, in the left of 40.16. These last figures may represent the contracting power of the muscles, and if so, the ability of the left arm is about 85% that of the right.

This difference in the extent to which the muscles are contracted is shown very clearly in my own case. This measurements are: right biceps contracted 330^{mm}, relaxed 275^{mm}; left biceps contracted 317^{mm}, re-

laxed 275^{mm}. The measurements are the same for the muscles in the relaxed condition, but when contracted the difference is 13^{mm}, in favor of the right. There is a difference also, in the contours and in the lengths of the two biceps. The right is shorter and more clearly defined, an indication that it has greater power of contraction.

Increase in endurance.

In the initial test the subject raised the weight until forced to stop on account of fatigue or pain, though the test was not carried to the very extreme of endurance. The number of flexions made is given in Table VII.

TABLE VII.

Number of flexions made.

		R.	*L.*			*R.*	*L.*
G.	Initial.	180	100	I.	Initial.	250	223
	Final.	1000	300		Final.	1000	250
	Gain.	820	200		Gain.	750	27
H.	Initial.	100	80	J.	Initial.	200	75
	Final.	1050	110		Final.	600	300
	Gain.	950	30		Gain.	400	225
F.	Initial.	100	125	K.	Initial.	136	93
	Final.	1000	200		Final.	860	607
	Gain.	900	75		Gain.	724	514

This table also gives the number of flexions made in the final test. The weight which in the initial test was heavy enough to reduce either arm to a condition of fatigue after a few minutes' work, was not able to so effect the right in the final test, though soon fatiguing the left arm. The practice had so inured the right arm to fatigue that with the given weight and time of flexion the work could be kept up almost indefinitely. At one thousand flexions the fatigue was scarcely noticeable, hence the test was not continued.

It is clear that the right arm had developed remarkably in endurance. K. who is slight and not of rugged health, at the end of the final test for right arm, said his arm was not tired in the slightest degree, although he was apparently almost overcome by the general effects of fatigue. The left arm also had gained in endurance though not to so great an extent. The average number of flexions for the left arm at the initial test was 119, at the final test, 297, a gain of 178 or 150%. We conclude that while the left had gained very materially, both in size and endurance, from the practice of the right, there was still a lack of that fineness of condition which seems dependent upon actual exercise of the muscle itself.

There is an entire lack of correspondence between the increases in

girth and in power of endurance. In fact the six subjects illustrate two types, differing widely in these respects. G., J. and K. belong to a type showing very little gain in girth, but exhibiting a marked increase in endurance of the left arm. H., B. and I. make large gains in girth for both right and left arms, but little increase in the endurance of the left arm.

TABLE VIII.

Comparison between girth measurements and power of endurance.

		Girth gains.		Flexion gains.
		Right biceps.	Left biceps.	Left arm.
	G.	5	—5	200
Type A.	J.	2	0	225
	K.	4	2	514
	Average	3⅔	—1	313
	H.	13	6	30
	B.	6	11	75
Type B.	I.	8	3	27
	Average	9	6⅔	44

In Type A (Table VIII.) there is an average gain of 3⅔^{mm} in the girth of the right arm and a loss of 1^{mm}, in that of left; while the flexions for left arm increase by 313. In Type B the girth gains are large, but the gain in flexions is only 44. The two types are, then, very clearly defined and separated from each other; while the similarity of the individual results in each type is quite close. For example, G.'s gain in right biceps is less than any in Type B. The same comparison can be made successfully with any individual whatsoever in either type. The facts noted are therefore worthy of the highest consideration.

Why such results are produced is not clear. Though it has often been noticed that a larger muscle is not always capable of greater strength and effectiveness than a smaller one, still one would think that there ought to be a correspondence in the same individual at different times between the size of his muscles and their endurance, and that if a marked gain in size occurred for any reason there should be also a marked gain in endurance. In Type A the nutrition has effected such a change in the muscle cells that they have gained endurance. In Type B the tissue that has been added may be fat, or some other constituent that has not been worked into the life of the muscle cell.

Increase in strength.

The strength of forearm was taken at both initial and final tests by means of an ordinary oval spring hand-dynamometer. The subject was given two trials with each hand and the highest mark made by each hand

revealed. No practice was given the subject in gripping the dynamometer, the object being to determine if there was any increase in strength of the right forearm due to general improvement in the nutrition of the arm. This being found, it remained to be determined whether there was a similar increase in the strength of the left. It has already been pointed out that as fatigue came on there was a certain amount of clenching of the hand in holding the dumbbell. One would reason that the muscles of the forearm would be developed and so an increase of strength would result. This is found to be true. Table IX. shows that this increase is

TABLE IX.

Increase in Strength.

		Pressure of dynam. in kilos. *R.*	*L.*			Pressure of dynam. in kilos. *R.*	*L.*
G.	Initial.	41.8	43.6	I.	Initial.	54.1	49.0
	Final.	45.4	40.8		Final.	56.8	53.1
	Gain.	3.6	— 2.8		Gain.	2.7	4.1
H.	Initial.	46.5	35.4	J.	Initial.	38.6	38.2
	Final.	54.1	48.9		Gain.	44.5	41.8
	Gain.	7.6	13.5		Final.	5.9	3.6
B.	Initial.	38.0	39.5	K.	Initial.	46.5	41.8
	Final.	53.5	50.0		Gain.	44.2	45.4
	Gain.	15.5	10.5		Final.	— 2.3	3.6
					Average gain	5.56	5.41

transferred to the left side. The average gain of the right arm is 5.56 kilos., of the left 5.41 kilos. or nearly as much. This corresponds closely with the figures obtained by Dr. ANDERSON, where the dynamometric pressure itself was practiced. He found, with practice of the right hand alone, a gain in the right of 11.7 pounds, in the left of 13.2, the gain in the left being the greater.

There were two failures to make gains in strength of grip. G., who, as we have before pointed out, neglected to follow instructions exactly, made no increase in the left forearm; and K., who showed no increase in girth of right arm, failed there also to increase in strength.

If we compare the gain in girth of forearm (hand clenched) with the gain in pressure, we find a close correspondence. The six subjects again fall into two types, not so clearly defined, however, as those in Table VIII. In Type C (Table X.) are placed H., B. and J., who show the largest gains in girth and also in the dynamometric pressure. Though there are some partial variations from the type, the averages prove the point very conclusively. The average gains in girth and in pressure in Type C are all large; in Type D they are all small, zero or minus.

A test of endurance differs materially from one of strength. The former requires a succession of small impulses for action, extending over a long period of time; the latter, a strong impulse for action for only a moment of time. The two tests are very unlike, and may require the development of entirely different factors.

TABLE X.

Comparison between girth measurements and dynamometric pressure.

		Girth gains. Forearm.		Dynamometer gains.	
		R	*L*	*R*	*L*
Type C.	H.	13	10	7.6	13.5
	B.	13	5	15.9	10.5
	J.	3	1	5.9	3.0
	Average	$9\frac{2}{3}$	$5\frac{1}{3}$	9.8	9.2
Type D.	G.	1	—2	3.0	—2.8
	I.	0	—4	2.7	4.1
	K.	—1	3	—2.3	3.0
	Average	0	—1	1.3	1.6

The effect of practice on the ability to resist fatigue and pain.

Some important facts were noted from the observations made in respect to fatigue, pain and soreness, due to exercise; and also in respect to the condition of the muscle, before and after practice. At the initial test the fatigue was local for both right and left arms, and was limited to pain in the attachments of the biceps muscles at the shoulder and elbow. The biceps itself did not tire. No general feeling of fatigue was experienced. For a few days after the initial test the muscles and tendons of both arms were very sore, so that practice was quite materially interfered with. No marked difference could be detected, by sight or pressure, between the muscular condition of the right and left arms.

At the final test fatigue was more general. K. was completely "tired out." Subject I. "ached in knees and back," was very nervous and could not sleep the first night after the test; his arms felt numb with a tendency to "go to sleep." J. experienced a great thirst during the test. When local pain was felt at all it was generally in the tendons, as at the initial test. Very little soreness either in the right or left arm was experienced as a result of the final test. When the muscles were pressed with the finger a slight difference could be noted between the condition of the arms, the right biceps being the firmer. The facts above

noted do not hold for all of the subjects. The data could hardly be exhibited in a table, so they have been summed up in general.

We may draw several conclusions from the facts observed in regard to fatigue, pain and soreness. (1) Practice so inured the right arm to its work that in the final test general fatigue came on before local fatigue. This hardening process was transferred in a striking degree to the unused side. In a work of endurance the tendons seem to weaken before the muscles themselves. (2) By practice the right arm reached such a condition that the after-effects of local soreness from continued exertion were avoided. This was found equally true of the unused side. (3) As far as could be judged from the examination by pressing the muscles with the fingers and from the amount of work done by both arms, the right arm had attained a fineness of condition not shared in by the left.

The immediate effects of exercise on girth measurements.

In order to determine if the blood circulation on one side of the body varied with the exercise of the other side, measurements of the biceps, contracted and relaxed, were taken in the order *R*, *L*, before the exercise

TABLE XI.

[illegible] before and after exercise.

		Biceps. 1st Meas.		Biceps. 2d Meas.		Gain over 1st.		Biceps 3d Meas.		Gain over 1st.	
		R	*L*	*R*	*L*	*R*	*L*	*R*	*L*	*R*	*L*
G.	C	288	269	293	268	5	—1	293	277	5	8
	R	232	226	245	228	13	2	240	240	8	14
H.	C	283	271	298	268	15	—3	290	273	7	2
	R	246	241	250	239	10	—2	251	246	5	5
B.	C	293	296	307	300	14	4	296	303	3	7
	R	255	256	267	255	12	—1	260	270	5	14
I.	C	338	320	348	320	10	0	347	330	9	10
	R	281	277	294	276	13	—1	291	292	10	15
J.	C	312	302	318	304	6	2	314	315	2	13
	R	264	262	274	260	10	—2	265	270	1	8
K.	C	280	262	290	261	10	—1	277	270	—3	8
	R	239	230	252	231	13	1	243	238	4	8
					Average C	10	1		Average C	$4\frac{1}{3}$	8
					R	12	—3		R	$5\frac{1}{2}$	$10\frac{2}{3}$

C—Contracted. R—Relaxed.

with the dumbbell; then in the order *R*, *L*, immediately after the test with the right arm; and finally in the order *L*, *R*, immediately after the test with the left arm. The measurements given in Table XI. do not show a corresponding variation in both members.

In the second set of the measurements there was an average increase of

10^{mm}, or 3%, in the girth of the right biceps contracted, and of 12^{mm}, or 5%, in the same relaxed. There was no increase in the left arm, but rather a decrease in size. The fact that the increase in right arm was greater in the relaxed condition, may be explained mechanically. The surplus of blood due to exercise was actually squeezed out of the muscle when it was rigidly contracted.

The increase in the size of the right arm is probably due to two effects of exercise: (1) the rush of blood to the muscle, shown by the distention of the superficial veins; and (2) the swelling of the muscle due to the production of heat and waste products. After K. had completed his test, a marked difference in temperature was noted between the biceps and the triceps. The fact was evident even to the sense of touch. The triceps felt cold in the comparison.

In the third set of measurements taken immediately after exercising the left arm, this member was found to have increased in girth, while the right, due to its quiescence, had already lost much of its former gain. In J. and K., who made the highest number of flexions with the left arm, the former increase made by the right is practically all lost.

Three facts would seem to show that the circulation of the side not exercised does not tend to vary in accordance with that of the side exercised: (1) the negative results in the left arm measurements taken after the exercise of the right; (2) the rapid decrease in girth of right arm, after its exercise, even though the left was then exercising; (3) the very manifest difference between the temperature of used and unused muscles in close proximity. The facts would indicate rather a variation of blood circulation in the opposite direction.

TABLE XII.

Amount of Practice.

	Total number of flexions.	Days of practice.	Av. flex's. per day.	Time of exercise.
G.	4900	12	408	Evening.
H.	5500	12	458	Evening.
B.	3100	12	258	Eve. and morn.
I.	3900	12	325	Eve. and morn.
J.	1050	18	115	Evening.
K.	6300	21	300	Morning.
Average	4125	14½	310	

Amount of practice.

For the practice experiments the instructions were not to tire or overwork the muscle but to exercise lightly and frequently. To show the

amount of practice done Table XII. was prepared from the subjects' notes. The duration of practice averaged 14½ days. The average member of flexions in each day's practice varied from 115 to 458, the total average being 310. The time of exercise was either early morning or late evening. No correlation could be discovered between the length and time of practice and the girth gains, or the number of flexions and the girth gains. The two subjects, J. and K., who continued the practice over the longest period of days made the greatest gains at the final test in the number of flexions of the left arm. The average of the periods of practice was 19½ days and their average gain in flexions, 369. For the other subjects the period of practice was 12 days and their gain in the left arm flexions only 53. This indicates that the transference of the effects of practice is not immediate, but occurs *after* the effects are noticeable on the side practiced.

TABLE XIII.

[illegible]

Subjects.	Conditions.			Results.							
	A	B	C	D		E		F		F	
				R	*L*	*R*	*L*	*R*	*L*	*R*	*L*
G.	28	12	408	5	—5	1	—2	820	200	3.6	—2.8
H.	26	12	458	13	6	13	10	650	30	7.6	13.5
B.	29	12	258	6	11	13	5	900	75	15.9	10.5
I.	26	12	325	8	3	0	—1	750	75	2.7	4.1
J.	26	18	115	2	0	3	1	400	225	5.0	3.0
K	24	21	300	4	2	—1	3	724	514	—2.3	3.6
Average	26[illegible]	14½	310	6[illegible]	2.[illegible]	4[illegible]	2[illegible]	757	178	5.56	5.41

A, subject's age.
B, practice period in days.
C, daily average number of flexions.
D, girth gain of biceps, contracted, in mm.
E, girth gains of forearm, contracted.
F, gains in number of flexions.
G, gains in dynamometric pressure, in kilos.

Summary.

Table XIII. is intended to exhibit at a glance the important conditions of the dumbbell test and the results obtained in relation to the amount of practice. For example, H., 26 years of age, during a period of 12 days, by lifting a weight of 2¼ kilos. with the right arm 458 times each day, increased the girth of the right biceps by 13^{mm}; of left by 6^{mm}. The averages give the results which may be expected with the given conditions. The figures would vary undoubtedly with another set of subjects or should any of the important conditions be changed. The general condition to be emphasized is that by practice of one side of the body

in muscular power the other side shares in the gain in size, strength and endurance.

IV. Accuracy of voluntary effort.

Lunging at a target with a fencer's foil was chosen as a suitable exercise to educate the subjects in accuracy and coördination. It is a complicated movement, involving the simultaneous action of many muscles and muscle groups. These coördinated muscles are in some cases remote from one another. Accuracy in the movement may be cultivated to considerable fineness. Attention is essential to such accuracy.

The lunge used in this work was that taught by the French school of fencing. It is described as follows: (1) The subject—when lunging right-handed—at the command "Ready," assumes a position with the right side to the target, left foot parallel to the target, right foot in advance a short step and at right angles to the left, knees slightly bent, body perpendicular, left arm bent over the head, right arm with foil in hand supine, right elbow bent and foil pointed at the target. (2) At the command "Lunge," the following movements are executed simultaneously: the whole body is thrown forward toward the target but the trunk is still perpendicular; the right foot is advanced a long step, but with knee still bent; the left foot is kept in its place and the left leg extended; the left arm drops to the back; the right arm is extended.

In this way the foil is advanced at the target. This lunge was taught as rapidly as possible during the practice. It required about a week for the subjects to learn it with any degree of grace and precision. They were allowed to practice the movements, right-handed, in their rooms but no records of accuracy were made except at the regular practice hour, in the presence of the investigator. The practice continued in most cases for 10 days.

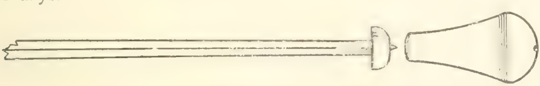

Fig. 3.

Apparatus.

A tack was soldered to the point of an ordinary fencer's foil. (Fig. 3.) The tack was filed to a sharp point of 3^{mm} length. A rubber cap was adjusted firmly over the end of the foil so that the point of the tack was hidden. When the foil was thrust against the target, the rubber was compressed so that the tack protruded enough to pierce the paper; the elasticity of the rubber, as the foil was drawn away, preventing any tear-

ing of the paper that otherwise would have been caused. The target (Fig. 4) was devised by Dr. Scripture. It was composed of two boards 60^{cm} square, and 2^{cm} and 1^{cm} thick. The lighter swung on the heavier one by a hinge at the top, thus forming a cover over it. A disc 47^{cm} in diameter was cut out of the thin board so that the center of the circle and the center of the boards coincided. At the ends of the perpendicular and horizontal diameters of the circle, four small nails with sharpened tops were inserted into the back board. Thin white wrapping paper was used for targets. To place a target the thin cover-board was raised, the paper was pressed down on the four nail points, the cover was lowered and firmly clamped. The whole target was

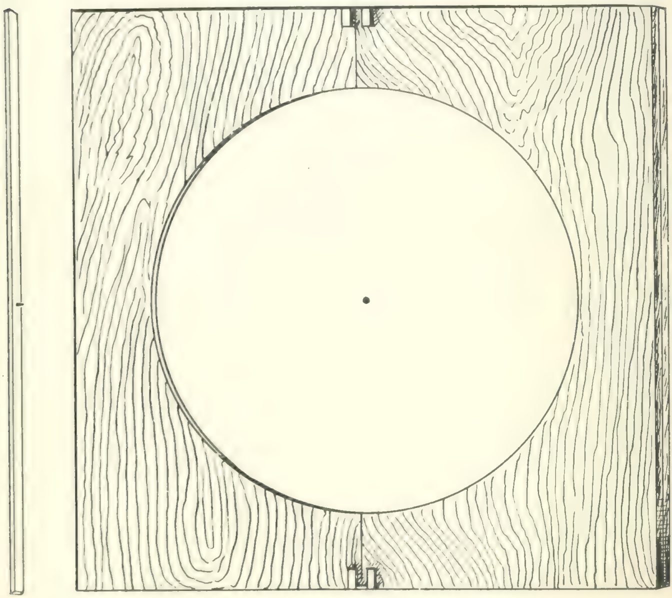

FIG. 4.

securely fastened against the wall at the height of $1\frac{1}{4}^{m}$. To center the target: a stick (Fig. 4) was fitted into two sockets on the cover. A nail with a sharp top was fixed in the middle of the stick, so that it

coincided with the center of the target. When the paper had been adjusted, the stick was placed in the sockets and by pressure on its upper surface the nail on its lower surface was driven through the paper into the center of the target. The stick was then removed, and a black pin 15^{mm} long, with a head $2\frac{1}{2}^{mm}$ in diameter, was inserted into the center to serve as a point to aim at. For each subject a distance from the target was chosen which seemed best adapted for his reach with the foil. This distance remained constant for him during the entire practice.

Characteristics of the subjects.

Four of the subjects were college freshmen, one a graduate student, and one a draughtsman.

Their individual characteristics may be indicated in the following manner :

I. Age, twenty-six. Graduate student. Temperament, nervous. Gymnast and athlete.

L. Age, seventeen. Temperament, nervous. Ambidextrous. General training in athletics.

M. Age, seventeen. Temperament, phlegmatic. No gymnastic training.

N. Age, twenty-two. Draughtsman. Temperament, motor. General training and especially in gymnastics. Ambidextrous.

O. Age, eighteen. Temperament, motor. No systematic bodily training.

R. Age, eighteen. Temperament, nervous. No special training except farm work

The average age of $19\frac{2}{3}$ years should be favorable for increase in accuracy and coördination.

Method of experiment.

For the test the subject was allowed either to dress in a gymnasium suit or to wear his ordinary clothes. In the latter case, however, the coat, collar and tie were removed to allow of free movement in lunging. After the manner of dressing had been chosen, it remained the same for each subject throughout the tests and all the practice exercises.

None of the subjects had fenced previously to the experiments. When the subject came into the room for the first time, the lunging movement was described carefully, but briefly, and each was allowed two or three preparatory lunges—right-handed—before a record was taken. Then, at the command "Ready," he assumed the position, and at "Lunge," he thrust the foil at the target. The observer stood ready with his pencil

and marked the hole in the paper with a figure 1. The lunge was immediately repeated until a record of ten thrusts was obtained. The paper was then removed and a fresh one placed in the target. Any important observations were also noted down. After five minutes the experiment was repeated with the left hand.

The subject was practiced for ten days in thrusting with the *right hand.* Ten thrusts were allowed for each daily practice. At the end of the practice period, the initial test was repeated to compare gains in the right and left arms. Notes were also taken in respect to improvement in *form* of lunging. The tests were all taken about 3 P. M., and for each subject all the conditions were kept as nearly as possible the same.

Results.

The figures tabulated in Table XIV. are distances in mm. from the center of the target and are the averages for ten thrusts. Subjects L. and N.

Table XIV.

Results in accuracy of lunging.

		Initial.		Final.		Gain.		Relative gain.	
	Age	*R*	*L*	*R*	*L*	*R*	*L*	*R*	*L*
I.	26	21.8	46.0	16.8	34.5	5.0	11.5	0.22	0.25
L.	17	36.2	39.9	25.4	27.2	10.8	12.7	0.29	0.30
M.	17	49.8	72.8	25.3	44.4	24.5	28.4	0.49	0.39
N.	22	56.2	50.4	25.7	35.4	30.5	15.0	0.54	0.29
O.	18	80.2	86.5	25.5	32.7	54.7	53.8	0.63	0.62
R.	18	59.2	47.0	27.5	41.9	31.8	3.0	0.53	0.06
Average	19.6	50.5	57.2	24.3	36.5	26.2	20.7	0.51	0.36

are practically ambidextrous, and in right and left initial tests they give average results not very widely separated. R. shows greater accuracy with the left than with the right although he is right-handed. In the final tests the right hand is more accurate in all cases. In three cases the left has made the greater gain in millimeters; in the other three, the right. Four subjects made greater relative gains with the right arm; two, with the left arm. The final averages show that the right arm is the more accurate. In the initial tests the average difference between right and left is 6.7^{mm}. In the final tests the difference is 12.2^{mm}. Hence the right has made the greater average gain, the difference being 5.5^{mm} in favor of the right.

Table XV. gives the probable error, expressed in millimeters and also in per cent. of subjects O. and I. The point to be noted especially is

that the probable error of the left arm has decreased as well as that of the right, though in each case the decrease is greater in the right.

TABLE XV.

Decrease of probable error.

		Probable error.		Probable error in %.	
		R	*L*	*R*	*L*
I.	In	12.46	12.02	15.5	13.9
	F.	2.86	3.62	11.2	11
O.	In.	31.2	4.92	14.3	16.9
	F.	1.84	3.74	10.9	10.8

Figs. 5 and 6 were constructed to illustrate the increase in accuracy

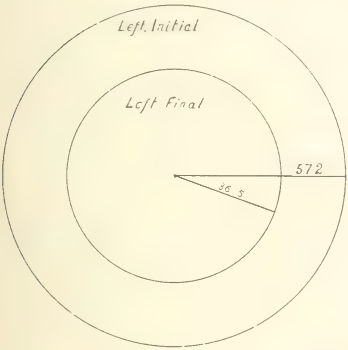

FIG. 5.

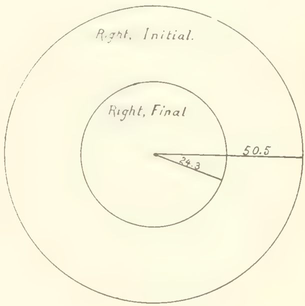

FIG 6.

due to practice in lunging. The cuts exhibit the facts given in Table XIV. The average distances from the center in the initial tests are indicated by the outer circles ; the distances in the final tests by the inner ones.

The effect of previous training.

The fact that the left does not gain so much as the right is emphasized by the consideration that, since the right arm is. to begin with, more accurate than the left, there is less opportunity for it to make large gains. There is a point beyond which increase in accuracy is extremely slow. The largest gains were made by subject O. whose initial records were the most inaccurate of all. The smallest gains were made by subjects I. and L. whose initial records were the most accurate.

Subject I., who is a trained man physically through gymnastics and athletics, made the most accurate average initial record. He was able

3

after 15 days' practice to lower the record of the right arm by 5^{mm}, and that of the left arm by 11.5^{mm}. M. and O., who were very inaccurate at the initial test, had had no special bodily training; at this particular test of accuracy they made great improvement. The others did not exhibit striking characteristics in the records; they were men of average bodily training. An exception to the preceding statement is to be made in the case of R., in whose case the left hand was superior to the right in the initial test and made only a small gain.

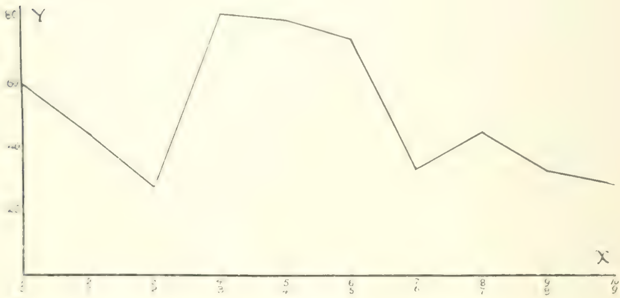

Fig. 7.

Fig. 8.

X, upper line, serial number of thrust.
X, lower line, successive steps in subject's practice.
Y, distance from center of target.

It is probable that accuracy, steadiness and coördination, when secured through any means of training, make their influence felt in any test that requires such qualities for its successful performance.

FIG. 9.

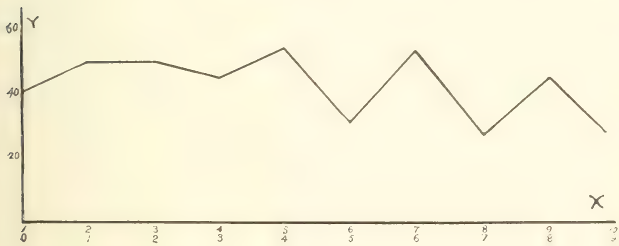

FIG. 10.

X, upper line, serial number of thrust.
X, lower line, successive steps in the subject's practice.
Y, distance from center of target.

Influence of practice on average accuracy.

The curves in Figs. 7, 8, 9, and 10 were made to illustrate the average accuracy of the successive thrusts in the initial and final tests. *X* indicates the number of the thrust in the series of ten, and *Y* the distance in millimeters from the center of the target. The initial and final records of subjects I., M., O. and R. were averaged to form the curves, the records of L. and N. having been cast out because these subjects were ambidextrous.

In Fig. 7 (initial test for the right hand lunge) the curve is irregular, reaching its greatest inaccuracy at the fourth thrust. At the final test for the right hand lunge (Fig. 8) the curve is more regular, but resembles the initial curve in still being most inaccurate at the fourth thrust. The curve for the initial test for the left hand lunge (Fig. 9) shows an increase in accuracy. The curve for the final test of the left hand lunge (Fig. 10) has been changed very materially by the practice of the right arm. The most important fact shown by the curves is that by the right arm's practice, both right and left have gained greater steadiness and uniformity. In the final tests each thrust is more nearly an average thrust.

Coördination of Movement.

Accuracy and coördination are closely connected. Accuracy depends in great part upon a delicate coördination of muscle groups and of motor centers. A few facts in respect to the subject's "form" during his performance of the lunges are here presented; the term "form" is used to mean the grace and precision with which the movement is executed.

When the foil was handed to the subject for the first time, he was instructed how to use it right-handed. After the short rest between right and left tests, he was asked to lunge left-handed, but no additional instructions were given. In most cases there was no confusion resulting from the change of side, though the form was not so good. At the final test the same points were observed, with the following results.

I. Movements with the left hand were executed correctly and in order, but a certain awkwardness was experienced.

L. (ambidextrous). Form almost perfect with the left hand.

M. Was not able during the practice to learn to dispose of the left hand properly. He always forgot to lower it when executing the lunge. This same trouble was experienced at the final test with both right and left hands. Otherwise the form was good, both right and left.

N. (ambidextrous). He remarked that it was easier to lunge left-handed than right-handed.

O. Form not so good left-handed as right-handed.

R. Not at ease left-handed. This shows in the records. Very little gain was made for the left arm.

In general, the movements necessary to the lunge are learned for the left side by practicing the right. Yet there is a perfection of grace and a fineness of coördination, that are attained only by the actual practice of the member under consideration.

Accuracy of skilled fencers.

The records of four experienced fencers, all of whom teach the art, were secured and are shown in Table XVI. Of these F.L., N.L. and G.M. teach with the foil in the right hand and have never fenced left-handed. WA. though right-handed, teaches with the foil in the left hand. For

TABLE XVI.

A comparison of the accuracy of skilled and unskilled fencers.

		Average accuracy. *R*	*L*	Average of *R* and *L*.
	F.L.	36.3	63.3	49.8
	N.L.	42.6	58.9	50.7
	G.M.	40.5	51.6	46.0
Average		39.8	57 9	48.8
	W.A.	24.9	23.8	24 3
Six subjects of Table XIV.	Initial.	50.5	57.2	53.8
	Final.	24.3	36.5	30.4

comparison with these records, the table gives those of the six subjects of investigation. The experienced fencers who had taught right-handed were not able to equal in accuracy the six subjects who had been practiced for ten days ; in fact they were not greatly better than those subjects at the initial test. W.A. is physically a perfectly trained man, and a teacher of gymnastics. These facts, together with his left-handed teaching, probably account for his accuracy. The inaccurate records of F.L., N.L. and G.M. may be explained in two ways. (1) In fencing, accuracy is not cultivated to any great extent,[1] but skill in parrying and the ability to get the thrust in at the right moment are considered the essential points. (2) Much of the accuracy of the six subjects is due to their practice under exactly the same circumstances. Possibly skilled fencers would be enabled to make unusually accurate records by a few days of similar practice.

Ratio of accuracy between right and left arms.

Table XVII. shows the ratios in accuracy between right and left arms. With one exception, the ratio has increased, as the result of the practice with the right arm. It is significant that the average final ratio of the six subjects is nearly the same as the average ratio of F.L., N.L. and G.M. For W.A. who fences with either the right or the left hand, the right and left sides are practically equally accurate.

[1] SCRIPTURE, *Tests of mental ability as exhibited in fencing*, Stud. Yale Psych. Lab. 1894 II 123.

TABLE XVII.

	Initial ratio.		Final ratio.	
	R	L	R	L
[illegible]	1	2.11	1	2.05
L.	1	1.02	1	1.07
M.	1	1.40	1	1.75
N.	1	0.89	1	1.37
O.	1	1.08	1	1.28
K.	1	0.80	1	1.03
[illegible]	1	1.13	1	1.50
F.L.			1	1.74
N.L.			1	1.38
G.M.			1	1.27
[illegible]			1	1.45
W.A			1	0.95

In the case of the skilled fencers, very little difference could be detected between the "form" of the right and the left sides. The fencers themselves were surprised to find it so easy to lunge left-handed. Not only was there no apparent awkwardness, but the new movement was executed with a considerable degree of precision and accuracy.

Fatigue.

The test was too short to allow the element of fatigue to enter to any extent. In many individual records, however, fatigue, or what produced similar effects, was present. Figs. 11, 12 and 13 are curves constructed from individual records. X is the number of the thrust in the series, Y, the distance of the thrust from the center of the target. The curve in Fig. 11, a practice record of subject L., shows a tendency to improvement in accuracy which is regular till the eighth thrust. After this, fatigue seems to come in and the accuracy decreases with the tenth. In Fig. 12 the curve of M. shows improvement till the fifth thrust, in which the center is struck. Fatigue appears earlier in this record and there is a general decrease in accuracy to the end of the record. In no case did the subjects complain of fatigue, but the observer could detect a wavering of the foil point toward the end of records like Figs. 11 and 12. Though the subject was not conscious of it, the fatigue was evident in the decrease of accuracy and in the unsteadiness of the foil.

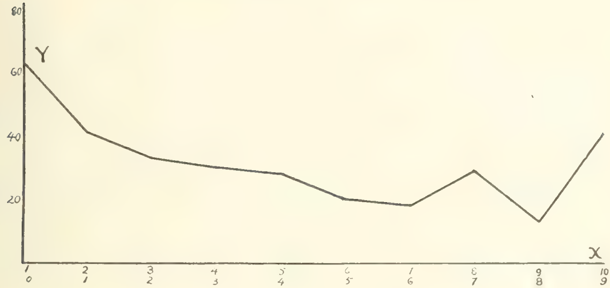

FIG. 11.

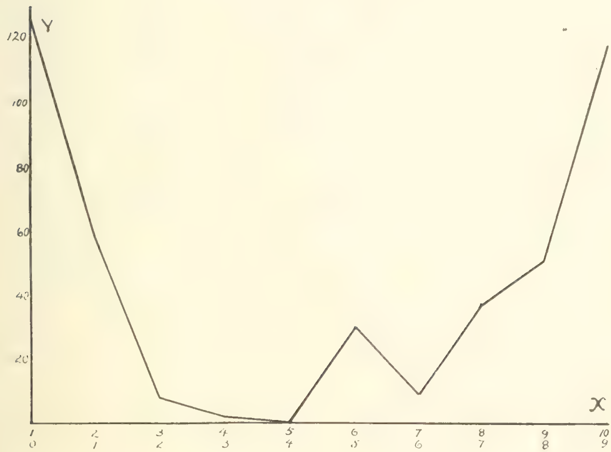

FIG. 12.

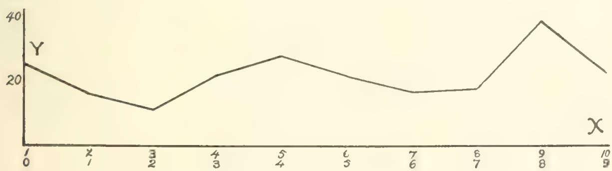

FIG. 13.

X, upper line, serial number of thrust.
X, lower line, successive steps in subject's practice.
Y, distance from center of target.

In N.'s fourth practice record, shown in Fig. 13, there is no great variation from the first thrust. After each thrust the subject's nervous equilibrium is reëstablished before the next thrust is made.

The form of any individual curve is dependent on three factors. (1) The condition of the nervous and muscular systems. The exercise of the nerves and muscles in lunging tends to improve their condition and hence accuracy is increased. If the system is in prime condition, due to other exercise just previous, this factor does not enter in largely. (2) Fatigue. This factor is more likely to affect the latter part of the curve. (3) Recovery. This factor is in opposition to (2). If the system is able to recover itself completely after each thrust the effects of (2) are counterbalanced. In Fig. 9 the average of left initial test, factor (1) is evident. The accuracy increases from the first to the last because the condition of the nervous and muscular systems is being improved by exercise. In Fig. 12, factor (1) is evident in the first part, factor (2) in the last part. In Fig. 13, factor (2) is counterbalanced by (3).

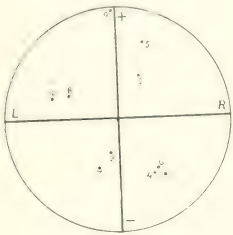

Fig. 13

Types of grouping.

When the records were measured the position of each thrust point was taken, and also its angle with a horizontal line passing through the center. The subjects were rather inclined to group their records in a particular manner. Fig. 14 illustrates a type of grouping that was generally given by N. It is the same record as that shown in Fig. 13, and is what may be termed an *average* record. There are 5 minus and 5 plus, 5 right and 5 left thrusts, and the accuracy is fairly constant.

Fig. 15

Fig. 15 illustrates a different kind of grouping. With the exception

of the 5th, the thrust-points are in a line, extending from $R+$ to $L-$. Two of the subjects were quite apt to make such groups.

In Fig. 16 are represented both right and left records of W.A., who is equally proficient with both hands. The right hand thrust-points are indicated by crosses, the left by dots. Line AB is drawn from $R+$ to $L-$ a little to the left of the center. All the right hand thrusts, with one exception, are above to the left of AB. All the left hand thrusts, with one exception, are below to the right of this line. A similar arrangement of thrusts was made by the other skilled fencers, though not in so marked a degree.

The following facts may be noted from a study of the groups: (1) Most of the groups are similar to Fig. 16,—right hand thrusts to the

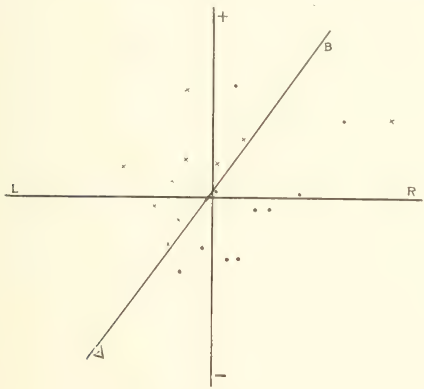

FIG. 16.

left of the horizontal line, left hand thrusts to the right. (2) The tendency to make such groups is more marked in skilled fencers. (3) The tendency increased in the six subjects during the practice. (4) The groups are most marked and distinct in the records of W.A., who has fenced with both right and left hands.

There is some evidence, then, to conclude that the influence causing the group records is unequal muscular development. The muscles chiefly used in the lunge are emphasized by practice and by their excessive contraction pull the arm to the other side of the body, thus producing a one-sided record. This unequal muscular development is transferred to the symmetrical muscles on the other side. The influence in

grouping is, therefore, peripheral and not central or mental. The chief influence at work in regulating the accuracy is central or mental.

V. Diffusion of Energy.

The following experiments were undertaken with the hope that they might help to furnish some explanation of cross-education.

Apparatus.

This was the same as that used in the tapping test first described, except that another key was clamped to the lap board, to allow one for each hand.

Method of experiment.

Records of five seconds of tapping were taken for the right hand, both tapping alone and also in connection with the other members. The records were taken in the following order: (*a*) direct, (1) right hand alone, (2) right and left hands together, (3) both hands and right foot, (4) both hands and both feet; (*b*) reversed, in the order (4), (3), (2), (1). In all cases only the right hand made a record. For each subject, two series of records were taken on each of four days. On the first and third days the records were taken in the direct order, and on the second and fourth days in the reverse order. When more than the right hand was tapping no attempt was made to have the different members tap with equal rapidity. The subject was instructed to devote as much attention to one member as to another and to tap as fast as possible with each. He did not know that the tapping of the right hand alone was being recorded. The seven subjects were Divinity and Graduate students of Yale University.

Results.

Table XVIII. gives the results obtained for the seven subjects. The figures here are made up from two averages. The average of two tests in the direct order was combined with that of two tests in the reversed order.

From the table it is evident: (1) that in general the right hand tapped more rapidly alone than in connection with the other members, (2) this difference in rapidity was not so marked on the third and fourth days as on the first and second. Even so short a practice made a striking change. In all cases, in the average of the first two days, the right hand was most rapid alone; except in the cases of H. and V., this difference was marked. In the average for the last two days, in all but two subjects, H. and V., the right alone was still more rapid though the difference in rapidity is small; on the contrary H. and V. were able to tap most rapidly

when all four members were tapping. The facts were generalized in the final averages. In the total average for the right hand alone the record was 36.9 for the first and second days, 36.3 for the third and fourth days, so that no gain in rapidity was made. The right hand's total average for tapping in connection with all the other members was for the first days, 29.7 and for the last, 34.7, a gain of 5 taps for 5 seconds, or 16%.

TABLE XVIII.

The influence of practice on automatic movements.

	1st and 2d days.				3d and 4th days.			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
H.	35	36	34	33	36	37	38	39
S.	27	28	21	18	35	32	32	33
T.	45	46	41	39	44	41	41	42
V.	38	35	33	36	33	34	35	36
X.	34	29	20	20	35	34	31	28
Y.	42	42	27	31	39	39	37	34
Z.	36	31	32	28	31	31	27	28
Total average	36.9	35.6	31.4	29.7	36.3	34.2	34.8	34.7
Relative average . . .	1.00	0.96	0.85	0.80	1.00	0.94	0.95	0.95

(1) Right hand alone
(2) Right hand with left hand
(3) Right hand with left hand and right foot.
(4) Right hand with left hand and both feet.

The last line of Table XVIII. shows the changes in rapidity that occurred with a practice of four days, giving the proportional rapidity of the right hand under the different conditions. In the tests of the first two days the right hand lost 20% of its speed while tapping with other members, in the last two days, only 5%. Fig. 17 represents the same facts in the form of a curve. *X* gives the number of members tapping, and *Y*, the number of taps in 5 seconds; while only the right hand is being recorded. The continuous line is the average of the first two days, the dotted line that of the last two days. There is a sharp descent in the continuous line, while the dotted line is more nearly straight. This indicates that practice has increased the ability of the right hand to tap in connection with the other members.

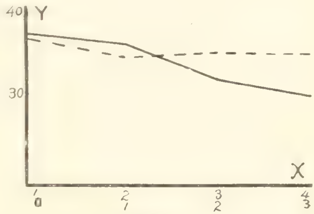

FIG. 17.

X, upper row, number of members used in tapping.

X, lower row, number of members in addition to right finger.

Y, number of taps in 5 seconds made by right index finger, the heavy line representing the average for the first two days of practice, the broken line that for the last two days.

With a longer practice the right hand, in multiple tapping, would undoubtedly excel in rapidity its record while tapping alone. This was true in the case of H. and V. who at first gave four records of nearly equal rapidity.

Regularity of tapping.

During the last tests of the experiment several of the subjects remarked that the multiple tapping had become easier, that they only needed to "set the machinery going and it went itself." The experimenter, listening to the tap counter, observed that in multiple tapping the right hand was more regular than when tapping alone. A specimen record is given to illustrate this fact.

	1st.	2d.	Average.	
(4)	39	36	37½	Tapping very regular.
(3)	36	35	35½	" " "
(2)	35	34	34½	Tapping irregular.
(1)	33	34	33½	" "

The complete record was taken in the reverse order, the observations appended being written down between the consecutive 5 second records. The irregularity is probably caused by too great concentration of energy or attention at one point—too much for the muscles to react to or properly dispose of. Hence the time was slower. It does not appear that the cultivation of the power to attend to several members at once is responsible for the increased rapidity in multiple tapping, since there was an actual decrease of attention in the later tests. For the same reason the increase of stored energy would not account for the fact. Very little energy is required during the short test, and one is able to tap continuously, without decrease in rapidity, for several minutes, while here there was a rest of 15 seconds between each 5 second record and the succeeding one.

The experiments of WELCH[1] give results similar to those described above. She concludes that rhythm of the left hand affects the right hand for the pull of the dynamometer, but that after practice this influence disappears. The explanation of such facts is probably found in the development of lower automatic centers by practice. After this development two acts, each of which at first requires conscious attention, become easy of simultaneous performance because the new automatic centers control the movements independently of the mind.

Fig. 17 exhibits the fact that in the first day's practice the right hand taps about as rapidly whether tapping alone or in connection with the

[1] WELCH, *On the measurement of mental activity through muscular activity and the determination of a constant of attention*, Amer. Jour. Physiol., 1898 I 283.

left hand, while much of its speed is lost while tapping with either one or both feet. This means simply that one arm is in closer nervous connection with the other arm than the upper limbs are with the lower.

VI. Theoretical.

An explanation of cross-education cannot as yet be completely and satisfactorily made. To aid in the explanation of the fact certain experiments of other observers are here brought together.

Experiments in diversion.

Lombard,[1] in his work with Mosso's ergograph, has shown that the strength of the right and left hands may vary either synchronously or independently. By means of two ergographs he secured records of both hands at the same time. He concludes that the variations are not due to any abatement of will power or attention, or to the fatigue of muscle; but rather to changes which affect the lower centers of the spinal cord. The variations in strength which occur synchronously are due to changes, probably circulatory, affecting large parts of the central system; those which occur independently are due to local changes.

Bryan's[2] experiments in tapping, carried on with children of different ages, prove that the effects of effort, through either extremity, are shared by both; and that the tapping ability of a joint is affected by the simultaneous exertion of the symmetrical joint on the other side.

Patrizi[3] found that the strength of one hand varies whether it works alone or with the other hand. In his tests with the ergograph he found: (1) that in simultaneous action, the right hand could do 3.67 kgm. of work, and left, 2.94, total, 6.61; in alternative action, right, 3.72, left, 3.58, total, 7.30 (it should be noted that the gain in alternative action is made chiefly by the left hand, as if, in simultaneous action, more attention were paid to the right hand than to the left); (2) that with the left hand in isolated action, its work was 4.63 kgm.; and with the left working alternately with the right, its work was 5.64—a gain of 1.01 or 21.8%—showing that the action of the right hand reinforces that of the left.

[1] Lombard, *Some alterations in the strength which occur during fatiguing voluntary muscular work*, Jour. of Physiol., 1893 XIV 113.

[2] Bryan, *On the development of voluntary motor ability*, Amer. Jour. Psych. 1892–93 V 201.

[3] Patrizi, *La simultanéité et la succession des impulsions voluntaires symétriques*, Archives Ital. de Biol., 1893 XIX 126, of which an abstract appears in the Année Psych., 1894 I 452.

The experiments of Welch,[1] as well as my own already described in the section on *Diversion of energy*, reinforce these researches in establishing the very close nervous connection existing between the right and left limbs.

Experiments on fatigue.

In Mosso's[2] experiments with the ergograph, results appear to prove that muscular and central fatigue are quite distinct and independent. Fatigue, if considered as decrease in capacity for work,[3] is due almost wholly to influences that are central. After complete volitional fatigue, the muscle may be made to do much more work by electrical stimulation either of the muscle itself or of the nerve leading to it. LOMBARD,[4] and ROSSBACH and HARTENECK[5] support Mosso in these conclusions.

Such considerations simplify the explanation of transference of practice; for if fatigue is chiefly central it is here that we must look for the most marked changes due to exercise. The effects of practice for the two hands, for example, are brought to an organ that has many things in common for both members.

Observations on attention and will power.

TOULOUSE[6] has proved the great importance of attention in acts of volition. The ability to pay attention decreases in all mental maladies. By means of the dynamometer he found that persons affected with diseases of the mind can exert only a feeble pressure, and he concludes that this inability is due to their lack of power to concentrate attention.

STUMPF,[7] in commenting on the investigations of FECHNER and VOLKMANN, described above on pages 6 to 7, makes the development of attention of great importance in transference. "The capability of concentrating attention on a certain point in question, in whatever field it is acquired, will show itself efficacious in all others." FECHNER, too, in this connection emphasizes attention.

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[1] WELCH, *On the measurement of mental activity through muscular activity and the determination of a constant of attention*, Amer. Jour. Physiol., 1898 I 283.

[2] MOSSO, *Ueber die Gesetze der Ermüdung*, Arch. f. Anat. u. Physiol., Physiol. Abth., 1890 89.

[3] SCRIPTURE, The New Psychology, 228, London 1897.

[4] LOMBARD, *The effect of fatigue on voluntary muscular contractions*, Amer. Jour. Psych., 1890 III 24, also *Effet de la fatigue sur la contraction musculaire voluntaire*, Arch. Ital. de Biol., 1890 II 380.

[5] ROSSBACH AND HARTENECK, *[illegible]*, Arch. f. d. ges. Physiol. (Pflüger), 1881 XV 2.

[6] TOULOUSE, *Notes sur quelques expériences dynamométriques chez les aliénés*, Soc. de Biol., 1893 V 121.

[7] STUMPF, Tonpsychologie, 1883 I 81.

GILBERT and FRACKER's recent investigations[1] of reaction-time for sound prove that practice in reacting to one form of stimulus shortened the reaction-time for other forms. Like results were found for the ability of discrimination.

SCRIPTURE's[2] hypothesis for explaining cross education is "physiologically speaking, that the development of the center governing a particular member causes at the same time the development of higher centers connected with groups of members. Psychologically speaking, development of will power in connection with any activity is accompanied by a development of will power as a whole."

Observations on mirror-writing.

SALTMANN,[3] PIPER[4] and TREITEL[5] have investigated the subject of mirror-writing, both in normal and abnormal persons and in children and adults. Their results show a greater tendency to reversed writing in the young and in those suffering with nervous disorders or sensory deficiency, notably in the blind, deaf and idiotic.

GOLDSCHEIDER,[6] in his review of SALTMANN's article, explains mirror-writing as due to the greater influence in some persons of motor sensations over optical percepts. He considers that two directing factors must be distinguished in the production of writing; the optical percept of the written sign and the motor sensations involved in the movements. In mirror-writing the motor sensations correspond to those in direct writing, but the written signs do not correspond to the optical percepts. The sequence of innervation occurs in this case under the influence of the motor sensations which appear to be dissolved from the optical percepts. These latter form the principal factor in general and with normal persons; passing through the series of innervations according to motor sensations, represents a lower, partly mechanical type.

GOLDSCHEIDER's review apparently explains mirror-writing and also the transference of the effects of practice. If in certain persons the

[1] GILBERT AND FRACKER, *The effect of practice in reaction and discrimination for sound upon the time of reaction and discrimination for other forms of stimuli*, Iowa Stud. Psych., 1897 I 62.

[2] SCRIPTURE, *Recent investigations in the Yale Laboratory*, Psych. Rev., 1899 VI 165.

[3] SALTMANN, *Schrift und Spiegelschrift b. gesunden u kranken Kindern*, Festschr. z. Henoch's 70. Geburtstag, 432, Berlin 1890.

[4] PIPER, Schriftproben von schwachsinnigen Kindern, Berlin 1893, reviewed in Zt. f. Psych. u. Physiol. d. Sinn., 1893 VI 74.

[5] TREITEL, *Ueber das Schreiben mit der linken Hand und Schreibstörungen*, Deut. Zt. f. Nervenheilk., 1893 IV 277.

[6] GOLDSCHEIDER, Zt. f. Psych. u. Physiol. d. Sinn., 1891 II 414.

motor sensations have a greater influence than the optical percepts, then by the transferrence of the motor sensations a movement is made on the other side that is symmetrical to the movement practiced. This symmetrical movement produces the writing in the reverse direction.

Reflex action.

WALTON[1] has shown the intimate connection between the centers of motion and sensation and between the motor centers for different groups of muscles. The muscles of a frog under the influence of strychnine may all be put into a condition of tetanus by the stimulation of only one point of the skin. In ordinary reflex action the motor center for the muscle of the eyelid must be closely connected with the sensory center for the cornea, as the stimulation of one causes a contraction of the other.

HOFBAUER[2] has proved by means of the ergograph that a stimulation of the sense of hearing may excite a muscle to greater action. A pistol shot at just the right moment causes a higher contraction than ordinarily; but if it occurs a moment too soon or too late the contraction is hindered.

Observations on the overflow of energy.

The researches of EXNER[3] in reflex and cortical stimulation in the dog establish several important facts. (1) The two methods of stimulation may reinforce each other. (2) A cortical stimulation which concerns only the left foot reinforces the reflex act which, it might appear, concerns only the right foot and its central organ. That is, if the motor area for the left foot is stimulated electrically and at the same time the right foot is stimulated for reflex action, the movement produced in the right foot is greater than if only reflex stimulation is used. It was found, too, that cortical stimulation of the area governing one front foot reinforces reflex action in the hind feet, and likewise the reverse.

URBANTSCHITSCH,[4] in his experience with pathological subjects with diseases of the eye and ear, has found true for the sensory nerves what EXNER found for the motor nerves. Sensations, as well as motor impulses to action, may affect parts seemingly not immediately concerned.

[1] WALTON, *Ueber Reflexbewegung des Strychninfrosches*, Arch. f. Anat. u. Physiol., Physiol. Abth., 1882 46.

[2] HOFBAUER, *Interferenz zwischen verschiedenen Impulsen im Centralnervensystem*, Archiv f. d. ges. Physiol. (Pflüger), 1898 LXVIII 546.

[3] EXNER, *Zur Kenntniss von der Wechselwirkung der Erregungen im Centralnervensystem*, Archiv f. d. ges. Physiol. (Pflüger), 1882 XXVIII 487.

[4] URBANTSCHITSCH, *Ueber den Einfluss von Trigeminus-Reizen auf die Sinnesempfindungen, insbesondere auf den Gesichtssinn*, Archiv. f. d. ges. Physiol. (Pflüger), 1883 XXX 129.

An hour's exclusive operation on the right eye showed on the left a relative enhancement of the ability to see. In many patients with chronic catarrh of the middle car the observer was surprised to find that an important pathological influence was transferred from the ear to the sense of sight.

DAMSCH[1] explains the spreading of nervous impulses to action as due to the close connection of all motor centers. Impulses from the motor centers tend to spread themselves by their entrance into the great central brain ganglion—where fibers from all motor centers come together and are intimately connected. This spreading of impulses is hindered by a checking, or inhibiting, apparatus which keeps the impulse from going the wrong way. This apparatus is much improved by practice. In the young and in certain nervously disordered persons it is deficient.

It was noticed in the tapping experiment that there was a tendency at times for the subject's left foot to make movements to accompany those made by the right. DAMSCH's explanation for such movements is that the impulse sent to the right foot has in part escaped through the checking apparatus and gone the wrong way. In learning an act that involves fine coördination it is very obvious that the pupil executes many movements that are entirely unnecessary. The nervous impulse has flowed out into wrong channels. These observations show how closely related are the motor centers governing symmetrical or associated parts and how an influence sent out from the central nervous system to a peripheral organ may be felt in other peripheral organs also.

Conclusions.

The following conclusions may be drawn from my own experiments and those of other observers.

a. The effects of exercise may be transferred to a greater or less degree from the parts practiced to other parts of the body. This transference is greatest to symmetrical and closely related parts.

b. There is a close connection between different parts of the muscular system through nervous means. This connection is closer between parts related in function or in position.

c. Will power and attention are educated by physical training. When developed by any special act they are developed for all other acts.

Explanation of cross-education.

With conclusions *b* and *c* established the explanation of transference is

[1] DAMSCH, *Ueber Mitbewegungen in symmetrischen Muskeln an nicht-gelähmten Gliedern*, Zt. f. klin. Med., 1891 XIX 170.

probably reached. There is no doubt that the most important effects of muscular practice are central rather than peripheral. The central effects may be distinguished as: (1) those dependent on the development of motor centers, that is, their improvement through exercise; (2) those dependent on the development of psychical factors, notably attention and will power. Of these two effects we would emphasize the first as the more important. In fact, in the tapping tests close attention and a strong will power were hindrances. In tests requiring strong effort these factors are useful.

With the improvement by exercise of the motor centers governing the right arm, there is through the close nervous connection an improvement also of the center governing the left arm. Besides this in tests where will power and attention are necessary, these elements are developed by the exercise of the right arm and are efficacious also for the left.

The peripheral effects of exercise cannot be ignored altogether. It has been noted before that in the dumbbell test, the left arm did no improve, relatively to the right, to so great a degree as it did in the other tests. It did not gain endurance to an extent comparable to the gain in the right. In purely muscular tests it is necessary not only to develop the center of motor control, but also to develop muscular tissue. The muscles must be put into better condition to gain endurance. It has been seen that the left arm gained in girth and to a varying extent in the power of endurance. This can be explained only by increased muscular nutrition.

The measurements to determine if the circulation varied alike in both arms when only one was exercised, gave negative results. That the centers governing the nutrition of the right and left arms are affected alike by the exercise of either arm, suggests itself as a probable reason for the increase in girth in the arm not used.

RESEARCHES IN PRACTICE AND HABIT,

BY

W. Smythe Johnson.

I. Triangular movement.

The subject was required to tap continuously at the corners of an equilateral triangle whose sides measured 20^{cm}. This triangle was formed by a special triangular contact key, Fig. 1, with knobs 20^{cm} from each other. This key was originally constructed at Dr. Scripture's suggestion for use in testing school-children by Gilbert, by whom, however, it was used merely for tapping and not as a habit-key.[1]

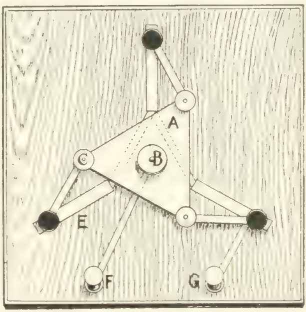

Fig. 1.

The key was placed in circuit with a 4 ampère battery and the primary coil of a spark coil, the condenser being connected around the break. From the poles of the secondary coil, one wire led to the base of the recording drum, the other to the base of a 100 v.d. electric fork bearing a flexible point on one of its prongs. Pressure on one of the key-knobs closed the primary circuit for an instant. When the circuit was broken a spark passed through the smoked paper on the surface of the drum making a dot on the time line drawn by the fork. Each spark thus indicated a tap on one of the three key-knobs. The time between the sparks could be read to the thousandth of a second.[2]

The subjects included: K. (Kochi) and M. (Matsumoto), who were Japanese students of psychology: P. (Powell), a student of English;

[1] Gilbert, *Researches on the mental and physical development of school-children*, Stud. Yale Psych. Lab., 1894 II 40 (especially p. 40 and Fig. 5)

[2] The arrangement of the recording apparatus was identical with that of Exercise IX in Scripture, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 113; it is shown in Scripture, New Psychology, frontispiece, London 1897.

L. (Lloyd), a student of philosophy; H. (Hawkins), a student of divinity; S. (Dr. Scripture), and J. (Johnson).

Before beginning the experiments the subjects were requested to devote their attention and energy to the exercise throughout each experiment and always to make as fast a record as possible. They were also instructed to practice no similar movements at any time outside that of experimentation. They were not allowed to inspect their records nor were they given the least intimation whether they were gaining or losing; exceptions to this rule occurred in the cases of J. and S.

The person experimented upon was placed in a quiet room.[1] The triangular key lay on a table. The subject stood while performing the experiment, as it was found that this position allowed the freest movement of the forearm. He fixed his gaze on the key. Before beginning an experiment a preliminary trial was allowed in order to acquaint the fingers with the relative positions of the keys. The subject was also asked to endeavor to regulate the tension of his muscles commensurate to the strain of tapping fifty successive times. Unless thus cautioned, he was almost certain to break down before the end. If he did not break down completely, he would at least miss some of the knobs in making the succession of movements. The caution was given only at the beginning of the whole set of experiments as it was merely intended to prevent the adoption of too high a standard in the beginning. It was not again mentioned lest the mental standard chosen in the first experiment should be changed. It was found that the subjects followed this request with respect to the left hand more closely than with respect to the right hand.

With all the subjects except S. and J. the experiment was first performed with the right hand, then with the left, and again with the right. In the case of J. only the right hand was used and in that of S. only the left hand.

Fatigue showed itself (1) when the subject completely broke down, (2) when he struck the knob of the key so inaccurately that he knocked it out of place, (3) when he missed one of the knobs of the key. The last case was the usual one and when it occurred the experiment was considered to have ended. By counting the records to this point we have a result practically free of fatigue.

Daily average.

The average tapping time for each day is shown in Table I. Curves

[1] Described by Scripture, New Psychology, 170, London 1897.

corresponding to the table are presented in Figs. 2, 3, 4, 5, 6. A comparison of these curves shows that, while each takes a direction determined by individual characteristics, yet they all follow closely the same law of gain.

TABLE I.

[illegible]

Subject.	Hand used.	Serial number of experiment. 1	2	3	4	5	6	7	8	9	10	11
K.	*R*	308	283	280	277	268	242	241	213	212	209	203
	L	300	292	316	278	276	258	243	230	234	230	226
M.	*R*	294	314	302	287	271	281	242	230	235	229	200
	L	314	321	302	288	262	281	274	273	253	250	187
S.	*L*	210	235	217	201	197	188	185	182	172	162	
J.	*R*	224	219	210	206	230	203	195	191	171		
P.	*R*	244	196	175	157	152	143					
	L	226	203	173	168	180	174					
L.	*R*	245	210	194	174	179	157					
	L	261	244	223	166	162	186					
H.	*R*	212	166	159								
	L	240	174	172								

The unit of measurement is $1\sigma = 0.001^s$.

The number of measurements in each case was from 40 to 50.

The probable errors of the determinations vary from $\pm 5\sigma$ to $\pm 1\sigma$.

An omission of one or more days did not in every instance materially effect the amount of gain resulting from practice. An interval of two days between the first and second experiments of P. and L. did not seem to effect their records; an interruption occurring between the fifth and sixth days with the subjects K. and M., and between the fourth and fifth days with J. and S., showed its effect on J. and M. very clearly, but in only a small degree on K., and seemingly not at all on S.

The losses on the fourth, fifth and sixth days with P. (left hand) may be attributed to the fact that he unthinkingly reversed the original direction of the movement in using the left hand. In the three previous exercises he had made his hand move clockwise. All the subjects except J. were right-handed and the right hand was always moved counter-clockwise, the left moving clockwise. The hands were thus moved symmetrically but in opposite directions. The subject J., however, moved his left hand counter-clockwise and his right hand clockwise. This would seem to indicate that the centers governing the rotary movements of the muscles in right and left handed persons are diametrically opposed

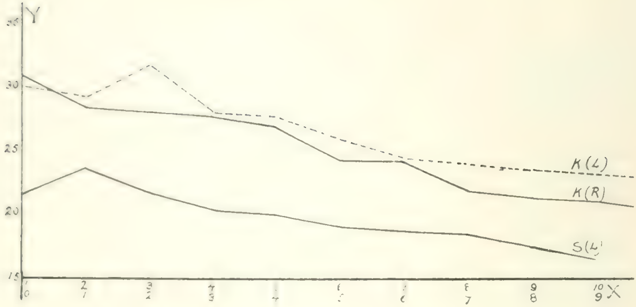

Fig. 2

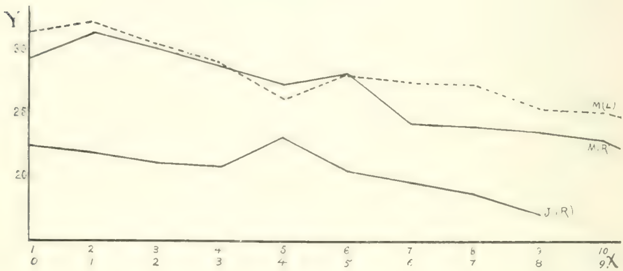

Fig. 3

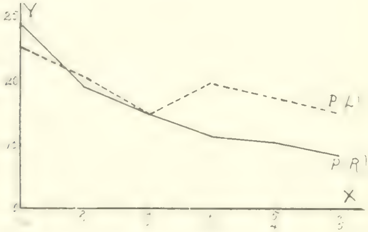

Fig. 4

X, upper line, serial number of experiment.
X, lower line, days of previous practice.
Y, tap time in thousandths of a second.

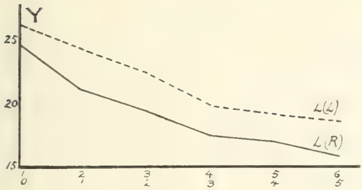

FIG. 5.

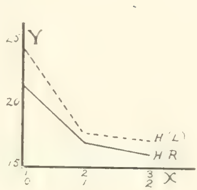

FIG. 6.

X, upper line, serial number of experiment.
X, lower line, days of previous practice.
Y, tap time in thousandths of a second.

to each other. A few experiments were made on another left handed person; the same direction of the movement of either hand was chosen as with J. However, I caused him to reverse the direction of the movement of the right hand so as to move counter-clockwise. As a result the movements of the right hand were much slower and fatigue set in earlier. Moreover, there was less regularity in the movements of the right hand.

Daily probable error.

The change in the probable error for successive days, given in Table II., may be considered as an expression of the development of automatic control over the movements of the hand.

TABLE II.

[illegible]

Subject.	Hand used.	Serial number of experiment. 1	2	3	4	5	6	7	8	9	10	11
K.	*R*	50	38	23	36	32	43	37	15	15	17	20
	L	80	51	18	38	30	29	20	16	27	40	18
M.	*R*	30	37	26	47	29	35	20	20	20	14	13
	L	35	38	19	20	24	30	22	26	27	17	42
S.	*L*	39	31	37	27	31	28	37	39	27	27	
J.	*R*	39	36	48	26	35	42	29	33	30		
P.	*R*	30	18	18	20	15	14					
	L	26	23	19	18	15	15					
L.	*R*	35	30	29	21	17	20					
	L	31	28	24	23	19	13					
H.	*R*	36	21	27								
	L	27	27	32								

The unit of measurement is $1^{\sigma} = 0.001^{s}$.

This table gives the probable error of the tapping times from which Table I. was computed.

The probable error P was calculated according to the formula

$$P = \sqrt{\frac{v_1^2 + v_2^2 + \ldots + v_n^2}{n-1}}$$

where $v_1, v_2, \ldots, v_n$ are the errors for the n measurements. Table II. shows a decrease from day to day which closely corresponds to the average daily decrease in the intervals between taps.

An average of the decrease in the probable error of all the subjects for

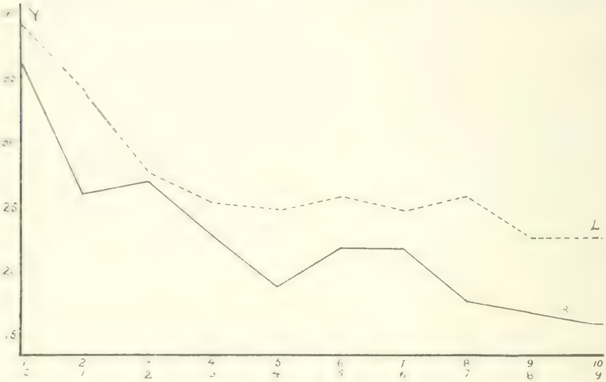

Fig. 7.

X, upper line, serial number of experiment.
X, lower line, days of previous practice.
Y, tap time in thousandths of a second.

each hand was made; a graphical representation of it is given in Fig. 7. Though the error for the left hand was larger than for the right, the curve for either hand takes about the same direction. Hence the centers governing the left-hand movements, though less developed, are susceptible to the same law of gain in automatic control.

The irregularities in the daily decrease of the error may be accounted for in part by the variations in the nervous condition of the subject from day to day. Moreover, the preliminary trials given just before beginning each experiment, which were always the same in number, were not sufficient in every case to arouse the nervous centers so as to get the best results from practice, for I observed that in a few instances the tapping time was very slow at first, but rapidly increased in speed during the first part of the experiment, which caused a larger probable error. Thus

the 43^σ probable error of K. on the sixth day was principally due to the long intervals between the first four taps.

That the preliminary trial was not sufficient in every instance for him to acquire momentum, so to speak, is seen in the record for the sixth day, which ran as follows: 315, 289, 291, 295, 200, 245, 244, 240, 269, 242, 248, 251, 243, 237, 246, 238, 254, 240, 229, 254, 218, 219, 260, 222, 231, 239, 235, 216, 223, 242, 229, 217, 212^σ. This shows a difference of 103^σ between the first and last taps, though the largest decrease occurred in the first four taps. As contrasted with this record, we give his record for the eighth day, which shows considerable gain in rapidity and regularity in two days' practice: 231, 229, 225, 220, 219, 223, 229, 226, 223, 221, 218, 222, 208, 210, 221, 226, 209, 204, 229, 221, 203, 207, 203, 200, 198, 217, 213, 209, 205, 202, 200, 195, 208, 203^σ. This record shows a difference of only 29^σ between the first and last taps. A graphical representation of the records for these two days may be seen in Fig. 8. According to the statement of the subject on the

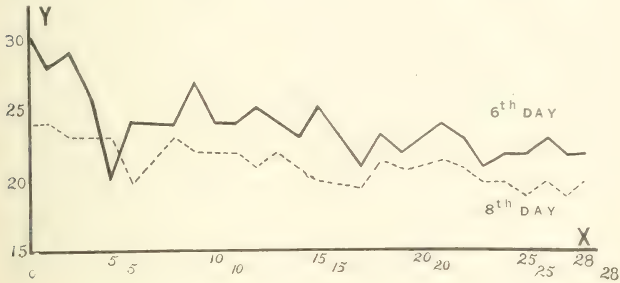

FIG. 8.

X, upper line, serial number of experiments.
X, lower line, days of previous practice.
Y, tap time in thousandths of a second.

sixth day, he was conscious at the start that he was not doing his best, but somehow he could not get control of the muscles within the time allowed him in the preliminary trial. Hence the temporary effect on the first part of the record.

In those records which show a small variability the subjects invariably reported that they were feeling quite well and brisk. Moreover, I observed that as the movements came to be controlled by the automatic centers and the movements became more rapid, the greater was the influence of any change in the nervous condition of the subject. An illus-

tration of this may be seen in the results for J., sixth day, where he reached the highest probable error of his whole set of experiments, having inadvertently received an electric shock just before beginning the experiment.

The rapid decrease in the intervals between taps with P. (right hand, Fig. 4) emphasizes the influence which previous training of any one set of muscles has upon the formation of automatic control where the form of movement is changed only as to direction. It is even more manifest in the decrease of the probable error. Although the movements involved in this exercise were different from those required in piano playing, yet the years of previous training had developed the centers controlling the movements of the hand to respond quickly and with considerable regularity: so when the movements became circular (as was necessary in tapping the three keys) it was only necessary to establish the one additional element in the automatic centers, the direction of the movement of the hand.

Averages of successive taps.

My observations during different experiments each day convinced me that the subject gained in rapidity of tapping constantly from the beginning of the experiment until the close or until the setting in of fatigue. I tested and corroborated my conviction in the following way: Reviewing the protocols, I derived an average from all the first taps, then from all the second taps, and so throughout the whole series. That my conviction was correct is clearly shown by the fact that in every case the tapping time was decreased, ranging from 30^σ to 100^σ.

Considering the large number of experiments made and the number of subjects included in these tests, the constant increase in rapidity of muscular action during each experiment exceeded our expectations. Indeed, it may be stated as a law of practice wherein rapidity is the objective point, the movement does not follow any rhythmical law of increase and decrease, but constantly increases in speed until the setting in of fatigue.

A few experiments were made wherein the tapping time was long continued, which showed that when fatigue first sets in, the subject loses for a short time, then there is a renewal of effort and the speed is constantly accelerated for a certain period, but not so long as in the first. As each successive period of fatigue came on, the successive period of increase in speed was shorter; this was continued until that state was reached where the alternations were so rapid as to effect almost every other tap. This accords with the assertions of other investigators who have directed their investigations especially to the study of fatigue.[1]

[1] [illegible], [illegible] fatigue, Stud. Yale Psych. Lab., 1895 III 68; Binet et [illegible], *[illegible] sse*, Année psychol., 1897 IV 267.

The lack of the development of the centers governing the movements of the left hand not only caused the tapping time to be slower than for the right hand, but the amount of gain during each experiment was also less. For instance, the average amount of gain for the right hand of K. was 90^σ in 20 taps, while with the left he gained only 40^σ. Stated in words, the gain with the left hand of all of the subjects was much less in proportion to the rapidity of its movement.

Although P. was the most rapid, yet he gained less in amount than the other subjects during each experiment, for the right hand gained only 50^σ and the left possibly 10^σ. This demonstrates the fact that, as the muscles come more completely under control, the influence of practice becomes less during each experiment. With L., however, the results for the left hand are more favorable, namely, a gain of 40^σ with the left hand, compared to a gain of 50^σ with the right. The results of H. are exceptional in that the gain was equal for both hands, namely, only 30^σ. With the subjects K., M. and P. the centers governing the left hand were slower in their activity and less influenced by training than with L. and H.

In like manner, the probable error for successive taps was determined. It showed a tendency to decrease as the experiment progressed. If the subject put forth the greatest effort in the beginning of the experiment, the error was correspondingly greater in the first part. But when the special effort was relaxed, the muscles reverted to their more accustomed speed of adjustment, and at the same time became more regular in their functioning. Therefore, a minimum gain in rapidity during any practice period is the best condition for impressing that standard upon the nervous centers; as a result the decrease of the probable error is correspondingly accelerated. This principle is well illustrated in the results obtained for the subjects J. and P. both of whom gained very little in rapidity of tapping during the progress of an experiment, but who, however, made considerable decreases in the probable error. As long as spasmodic accelerations are observable throughout a short series of practice, the automatic control over the muscles may be considered imperfect, the degree of imperfection being indicated by the amount of the probable error.

Relative gain by practice.

Although the gain from day to day varies in response to the mental and physical condition of the subject, yet we may suppose from the conformity of results of all the subjects that the acquirement of muscular facility, or, physiologically, the transition from that state which demands

constant fixation of the attention to the state of automatic control follows as closely a mathematical law as do falling bodies. The difficulty in finding such an expression is partly due to the difficulty in getting the subject under the same physical and mental conditions at each experiment and partly to the lack of scientific results on allied subjects upon which the law of habit must also depend. For instance, some of the personal factors of our problem causing variations in the results obtained from different individuals are: differences in muscular memory for different individuals; different physical conditions due to differences in the constituents of the blood, etc.; the rapidity of the heart-beat; the temperature of the body; the power of the fixation of the attention; the interest in the experiment; and the influence of emulation. The more nearly these various conditions approach the normal and the more accurate the measurements are, the nearer will the results for different individuals conform to the same law of gain in rapidity and regularity of muscular adjustments. This law of development may receive mathematical expression either by percentages or by algebraical formulæ. Of these two methods, I have adopted the former. I have adopted a method somewhat similar to that used by AMBERG[1] for determining the percentage of gain by practice. It will be seen, however, that I do not adopt his method without modification.

We took the average tapping time at the first experiment as a measure of the subject's ability without any previous training. The difference between the averages of the first and second experiments was taken as representing the influence of practice during the first experiment. Likewise, the average tapping time of the third experiment was subtracted from the average of the first. This process was repeated throughout the series. Then taking the sum of the whole number of gains over the average of the first experiment and dividing by the number of experiments, we obtained the average gain for the whole series over the results of the first experiment, which was expressed as a percentage of the average tapping time at the first experiment. But this did not complete the series, for at the succeeding experiment he began the practice anew just as at the first experiment. It was, therefore, necessary to repeat the same calculation, taking the average tapping time at the second experiment as the basis. In like manner, we continued the process for the whole number of experiments, thus obtaining the average percentage of gain, taking each succeeding experiment as the beginning of a series. Then dividing the sum of these percentages by the number of experiments

[1] [illegible] 1896 I 30.

we obtained the average percentage of gain for the whole series of experiments.

The formula for the computation of these percentages may be stated in the following way.

Let the averages for the 1st, 2d and 3d, $\cdots$, nth days be a, b, c, $\cdots$, l. Then take

$$
(1)\quad
\begin{aligned}
(a-b)+(a-c)+(a-d)+\cdots+(a-l)&=A,\\
(b-c)+(b-d)+\cdots+(b-l)&=B,\\
(c-d)+\cdots+(c-l)&=C,\\
&\ \vdots\\
(k-l)&=K,
\end{aligned}
$$

$$
(2)\quad
\begin{aligned}
A\div(n-1)a&=M\%,\\
B\div(n-2)b&=N\%,\\
C\div(n-3)c&=O\%,\\
&\ \vdots\\
K\div k&=Z\%;
\end{aligned}
$$

$$
(3)\quad \frac{A+B+C+\cdots+K}{(n-1)a+(n-2)b+(n-3)c+\cdots+k}=\text{Ave. }\%.
$$

The signification of such percentages is that they give us a true standard for the comparative influence of practice on different individuals. Although all practiced the same amount each day under similar conditions, yet we shall now see how differently the percentage of gain in speed of voluntary movements differed with each subject from day to day and how similar were the final results after the completion of the entire series.

TABLE III.

Relative gain in speed from day to day.

Subject.	Hand used.	1	2	3	4	5	6	7	8	9	10	Ave.
K.	*R*	0.21	0.16	0.17	0.18	0.18	0.11	0.13	0.02	0.03	0.03	0.12
	L	.14	.12	.21	.12	.13	.09	.04	.04	.03	.02	.09
M.	*R*	.12	.19	.17	.16	.12	.18	.06	.07	.07	.10	.12
	L	.16	.18	.14	.12	.05	.12	.12	.16	.14	.25	.14
S.	*L*	.11	.20	.15	.10	.10	.06	.07	.08	.06		.10
J.	*R*	.09	.08	.05	.04	.17	.08	.07	.10			.08
P.	*R*	.32	.20	.14	.06	.06						.16
	L	.17	.10	—.07	.09	.07						.07
L.	*R*	.26	.17	.13	.07	.07						.14
	L	.20	.18	.14	.05	.03						.12
H.	*R*	.22	.06									.13
	L	.28	.01									.14

The values in columns 1, 2, ..., 10 were calculated by formula (2), those in the column Ave. by (3). The figure for any particular day indicates the combined relative gain for all succeeding days over the record for that day.

The relative average daily increase in speed is given in Table III. For the right hand of K. the average percentage of gain on the second day was 21% over the speed made at the first experiment. But, as is shown in the table, the percentages decreased perceptibly until the close of the series of experiments, ending on the eleventh day with a gain of only 3% over the speed of the tenth day. The small percentage in the latter part of the series would seem to indicate that he had approximately reached his limit in rate of movement. So with all the other subjects, the percentage of gain in increase of speed constantly declined as the practice was continued from day to day. The large percentages in the first part of the series show that the greatest gains are to be made in the early part of practice.

The average percentage of gain given in the column of averages shows the comparative value of practice for each individual. Comparing K. and M., whose experiments extended over the same number of days, we see that practice had the same effect on the right hand of each of them, namely, 12%; but for the left, K.'s average percentage of gain was less than for the right hand, while with M. it was even larger than for the right. So with S. and J., we see that practice was of more value for S. than J. by 2%. Likewise P. gained 2% more with his right hand than did L.; but with the left hand L. gained 5% more than did P.

The average daily decrease of error was also derived according to the formula given on page 61; the results are given in Table IV.

TABLE IV.

Relative average daily decrease of error.

Subj.	Hand used.	1	2	3	4	5	6	7	8	9	10	Ave.
K.	*R*	0.45	—0.22	0.28	0.27	0.23	0.51	0.55	—0.16	—0.23	—0.18	0.15
	L	.64	.42	—.68	.29	.11	.10	—.27	—.9	—.7	.55	.10
M.	*R*	.13	.33	.05	.54	.30	.52	.16	.21	.32	.07	.26
	L	.25	.33	—.37	—.33	—.14	.11	—.27	—.11	—.9	—.59	—.11
S.	*L*	.19	—.02	.17	—.17	—.02	—.13	.16	.31			.05
J.	*R*	.11	.04	.32	—.30	.04	.27	—.09	.09			.07
P.	*R*	.43	.07	.09	.27	.07						.19
	L	.32	.26	.23	.17	.00						.19
L.	*R*	.38	.27	.33	.12	—.18						.18
	L	.31	.29	.23	.30	.32						.29
H.	*R*	.33	—.29									.02
	L	—.09	.10									.11

The explanation is the same as for Table III.

Comparing the decreases of the probable error for K., 15%, and M., 26%, we see that practice was more beneficial for the right hand of M. than

for the right hand of K. But for the left hand the reverse was true. Even if we cast out the last practice of M., on account of its poor effect, the — 11% changes to + 6%. It should be noted that the percentages for the left hand of K. and M. are the reverse of those given in Table III. Likewise the same is true for S. and J., for while S. gained more in speed, J. gained more in regularity of movement. The small percentage of decrease of the probable error would seem to indicate that S. and J. kept the order to tap as quickly as possible in the foreground of the attention, for the percentage of gain in rapidity was larger for the whole series with each of them than was the reduction of the probable error. On the contrary with the subjects K. and M. who had approximately reached their utmost speed of voluntary movement, the percentage of the decrease of the probable error was larger than the percentage of gain in speed.

We may summarize the results given in Tables III. and IV. in the following way: P. made the greatest percentage of increase in speed with the right hand, and S. and L. the greatest with the left; that of all the subjects J. possessed least ability for development of rapid movements with the right hand. When regularity, not rapidity is considered, the right hands of K. and M. and the left hands of K. and L. made the greatest gain in regularity of movement while S. made the least of all.

Relative average daily gain in speed and decrease of error compared.

The average percentage of increase in speed and of decrease in the probable error from day to day for all the subjects were compared; the results are expressed in the A and B curves of Fig. 9. These curves

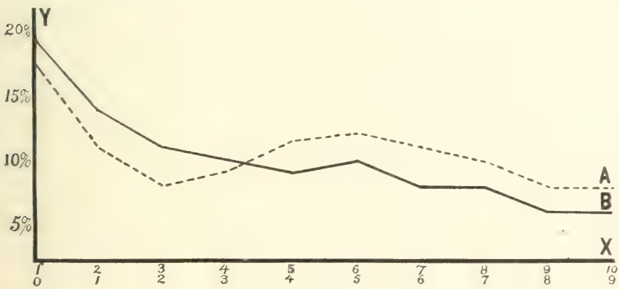

FIG. 9.

X, upper line, serial number of day.

X, lower line, days of previous practice.

Y, relative increase in speed (B) and decrease in probable error (A).

show very clearly that whereas the percentage of gain in rapidity of movement is larger in the first part of the series of the experiments, in the latter part the greatest influence is directed toward the reduction of the irregularity of the movements.

Considering the curves A and B as representatives of the influence of practice on seven subjects, indicating the gain made in six to eleven days, we are justified in making the following general statements concerning muscular action.

First, the gain in rapidity and the gain in regularity of muscular action are greatest during the first periods of exercise ; one or both of these continues to diminish as practice continues at each experiment as well as at successive experiments.

Second, during the earlier periods of exercise practice has greater effect upon the rapidity of muscular action, but later its greater effect is in the reduction of the irregularity of voluntary movement.

Third, the relations of rapidity and irregularity are largely affected by the relative complexity of the muscular movements, the number of muscles undergoing training, and the subject's power of concentration of attention.

II. Drawing Circles.

The object of this set of experiments in drawing circles was to show : (1) the gain in the reduction of imperfections in the drawings during each experiment and from day to day ; (2) the influence that a copy placed before the subject had upon the size of his drawings.

A package of ten sheets of paper, 125^{mm} by 100^{mm}, was placed before the subject, sitting at a table. A sheet of paper with a circle of 60^{mm} in diameter, described with a compass, was placed before him as a copy ; he was asked to look carefully at it each time before commencing to draw his circle. The copysheet was the same size as those used by the subject. The ten slips of paper lay one upon the other at the start ; the experimenter stood by and removed them as the circles were drawn. Thus all visual comparison with previously drawn circles was prevented. It was impressed upon the subject that he should keep the true circle constantly in memory after he took his eyes off the copy to direct his hand. The circles were drawn with the free hand, no portion of the hand or arm being allowed to rest on the table during the process. No restrictions were imposed on the time consumed in the drawing of the circles. All distracting influences were removed. Before beginning, the subject was enjoined to do his best at each trial.

The subjects were J. F. and S. F., teachers in the primary grades of

the New Haven city schools, who had previously had considerable practice in drawing circles in the school-room ; M. C., a girl of twelve ; B., C., H. and J., students in the university.

A characteristic expression for the deviation of the drawn curve from the true circle may be found by comparing the longest diameter with the shortest one. The two diameters were measured in millimeters and their difference was considered as the amount of error.

It was deemed advisable to limit the number of circles drawn at one sitting to ten, so that the element of fatigue might be almost if not completely eliminated. Preliminary experimentation showed that the influence of fatigue was not perceptibly present with the right hand until the ninth or tenth repetition, and with the left hand until the fifth to seventh repetitions. However, those curves in the series which most nearly approximated true circles resulted in attempts ranging from the fourth to the seventh. Consequently, under ordinary circumstances, the drawing of five circles would give sufficient practice for the subject to reach an approximate maximum of accuracy of adjustment. Hence it was deemed best to stop the experiment at a point where the subject was gaining in accuracy rather than to continue it until the errror began to increase.

Gain on successive days.

The average errors for each day are shown in Table V.

TABLE V.

Average error on successive days.

Subject.	Hand used.	Serial number of experiment. 1	2	3	4	5	6
B.	*R*	12	9	12	11	8	9
	L	10	9	7	5	5	8
M.C.	*R*	10	9	7	7	6	5
	L	13	10	7	6	5	4
C.	*R*	6	6	5	4	4	3
	L	10	9	9	9	8	7
J.F.	*R*	6	6	5	5	3	2
	L	11	10	9	8	8	4
S.F.	*R*	6	6	6	5	4	4
	L	7	6	5	5	4	4
H.	*R*	9	9	8	7	7	6
	L	7	8	10	8	9	10
J.	*R*	11	6	5	5	4	3
	L	9	13	8	12	7	6

The unit of measurement is 1^{mm}.

Number of experiments each day, 5 with either hand.

The probable error of a determination varies from $\pm 1^{mm}$ to $\pm 0.1^{mm}$.

As is shown in these figures, practice with the left hand gave more irregular results than that with the right. However, B.'s record was an exception in that the error was not so large as that for the right hand. Moreover, the error decreased more rapidly in the practice with his left hand. The small and irregular gain with his right hand may have been due to a tendency to accomplish the task with great rapidity and to previous practice in writing, for his penmanship was sharp and pointed. Hence the left hand had the advantage over the right since the right hand not only had to form a habit for a certain movement but to reform one.

With H. and J. the lack of improvement with the left hand was due to a decrease of effort. I observed that they made great effort on the first day; but after that they declared it useless for them to practice with the left hand because improvement was hopeless. These two records plainly demonstrate the influence of confidence in one's ability upon one's development. Purposeful attention and persistent effort on the part of the subject are the two most essential elements in practice for the establishment of any definite mode of muscular action.

Though the amount of gain with C. was the same for each hand, yet the magnitude of the error at the beginning of the series with the left hand gave more scope for improvement, and had the subject been equally skillful with his left hand the curve would have descended much more abruptly. On the contrary, M.C. gained more with the left hand than she did with the right. The very regular decrease of the error with this subject from day to day shows the influence of persistent effort; I observed that she was very careful throughout the whole set of experiments.

Though there was a gain of 7^{mm} for the left hand of J.F. as contrasted with a gain of 4^{mm} for the right, yet most of the decrease in the error for the left hand occurred at the last experiment, which was undoubtedly due to the extra effort put forth. Though the subject thought all along that she was doing her best while practicing with the left hand, yet after the close of the series she said that she was conscious of having made considerably greater effort at the last experiment. None of the subjects save J.F. knew that the sixth day's trial would close the set of experiments.

Gain in successive circles.

The average of the errors for all first circles, that for all second circles, etc., were taken. The results, Table VI., show that with the most of the subjects the error became less as the practice was continued.

TABLE VI.

Average error for successive circles.

Subject.	Hand used.	Serial number of circle. 1	2	3	4	5
B.	R	11	10	9	9	10
	L	8	5	9	7	9
M.C.	R	10	9	8	7	7
	L	7	6	6	7	8
C.	R	6	5	5	5	4
	L	10	9	9	8	7
J.F.	R	6	5	5	4	3
	L	9	9	8	7	7
S.F.	R	6	6	5	5	3
	L	7	6	6	4	4
H.	R	8	7	7	6	6
	L	9	8	7	9	10
J.	R	7	6	6	5	4
	L	6	9	10	8	10

The unit of measurement is 1^{mm}.
Each figure is the average of 6 experiments.
The probable error of a determination varies from $\pm 0.9^{mm}$ to $\pm 0.1^{mm}$.

It has been proposed by Dr. Scripture to call the curve of change for a single continuous experimental session the "curve of practice" and the curve of change for successive sessions the "curve of habit." In the present case the gain on successive days would be represented by a curve of habit and the gain in successive circles by a curve of practice.

In the case of M. C. the practice and habit curves are alike in that they each begin with an error of 10^{mm} and end with 7^{mm}. The maximum decrease of error was not reached until the fourth circle was drawn with the right hand, while it was reached in the second with the left. The control of the movements of the left arm was evidently not sufficiently developed to prevent fatigue during the time required to make five circles.

The similarity between the practice and habit curves seems to indicate that the development during each practice period follows closely the same law as does the daily progress, for in both alike the gain is most rapid in the first part of the exercise. However, the amount of gain for the different subjects varies considerably; this I attribute to the differences in the muscular memories of different individuals. With some, the error of the first circle on successive days was only slightly larger than the average for all the circles made on the preceding day, while with others it was in some instances equal to that at the beginning of the preceding experiment but rapidly decreased. The law of practice and the law of

habits are presumably of the same general form with different constants for different circumstances.

Comparison of the drawn curve with the original circle.

In order to indicate the size of the curve drawn as a circle, four diameters were measured and their average taken. The diameters measured were the shortest, the longest and the two 45° from these. These measurements furnished an approximate estimate for determining how much change, if any, there had been in reproducing the copy circle of 60^{mm} in diameter.

TABLE VII.

A. [illegible]

Subject.	Hand used.	Serial number of circle. 1	2	3	4	5
B.	*R*	62	64	66	63	62
	L	59	62	60	62	60
M. C.	*R*	64	61	58	57	54
	L	61	58	59	59	62
C.	*R*	60	61	62	64	68
	L	58	62	61	65	69
H.	*R*	61	60	58	57	55
	L	62	63	64	63	62
J.	*R*	61	62	63	65	68
	L	58	59	61	60	60
S. J.	*R*	61	62	64	65	67
	L	61	62	62	63	64
J. F.	*R*	63	58	60	60	60
	L	63	63	65	60	63

B. *Daily average size of circle.*

Subject.	Hand used.	Serial number of experiment. 1	2	3	4	5	6
B.	*R*	65	59	62	63	67	65
	L	57	61	61	62	63	60
M. C.	*R*	66	62	57	55	55	54
	L	64	61	61	60	57	56
C.	*R*	61	62	63	63	65	65
	L	61	62	62	64	64	65
H.	*R*	60	58	58	57	57	56
	L	61	63	63	62	63	63
J.	*R*	68	65	64	63	63	62
	L	59	63	57	63	61	55
S. J.	*R*	70	65	65	63	62	60
	L	70	64	61	59	59	58
J. F.	*R*	62	61	60	60	60	60
	L	66	64	63	62	60	62

The unit of measurement is 1^{mm}.

In section A, each figure is the average of six experiments.

In section B, each figure is the average of five circles.

The probable error of a determination varies from $\pm 2.1^{mm}$ to $\pm 0.3^{mm}$.

There were two points to which the subject was required to direct his attention in making each figure: the roundness of the figure and its correspondence in size to the copy. We shall now answer the question whether the subject directed his attention more especially to the former or the latter point.

The results given in the above table show three types of practice: (1) that in which the subject decreased the size of the circle both during the progress of each experiment and from day to day, (2) that in which the size of the circle was increased during the experiment but decreased from day to day, (3) that in which there was but little variation either during the experiment or from day to day. The first two classes characterize those who regarded more carefully the smoothness of contour of their own drawn curves than they did the correspondence in size to that of the copy. The third class were those who directed their attention more especially to the size of the curve, and who observed the copy closely each time before beginning to draw their own curves.

The measurements for B. showed that during the first three experiments he began by making the circle smaller than the copy, but increased its size during the experiment until it was larger than the copy, whether the right or the left hand was used. During this period he made the circles by moving each hand counter-clockwise. During the practice of the last three experiments, for some unknown reason, he reversed the direction of the movement for each hand and as a result the size of the successive circles was constantly decreased. The subject was not conscious of having made any change in the direction of the movement of either hand, and was unable to say which way he had moved his hand in the last experiment until he had made the motion as if making a circle, so unconsciously was the movement performed. Questioning the subject at the close of the series brought out the fact that this reversal may have been due to practice on the inverted oval as seen in the capital *W*. Excepting the change in the size of successive circles with the change in the direction of the movement of the hand, all the changes occurring during any one practice may be easily accounted for. If, for instance, after making the first circle, he judged that one to have been smaller or larger than the copy, then in the following he proceeded to correct it.

The figures given in Table VII. show that he made greater effort to approach the copy when practicing with the left hand than he did with the right. Moreover, I observed that he regarded the copy more carefully each time when practicing with the left hand. The care directed toward accuracy of contour and size of circles drawn with the left hand caused the time consumed in making the circles with the left hand to be about one-fourth longer than with the right hand.

With M. C., the right hand was directed counter-clockwise, as was the case with all the other subjects save B., already mentioned; the left was moved in the opposite direction. Starting with a diameter of 71^{mm} for the first circle with the right hand, she reduced the diameter to 61^{mm} during the first experiment. Likewise the decrease in the size of successive circles continued throughout the series of experiments, becoming less, however, at each successive experiment. The average of all the experiments, Table VII., shows a decrease of 10^{mm} in the size of successive circles; and in the daily averages there was a total decrease of 12^{mm}. These figures show that the subject decreased the size of the circle with the right hand both during the process of the experiment and from day to day. Likewise the average of successive circles for the left hand shows that the size of the circle was decreased in the second circle, but increased thereafter. The decrease of the size of successive circles for the left hand was not so large as for the right, which indicates that the subject directed her attention more especially to the size of the circle when practicing with the left hand, and to the smoothness of contour of her own curves when practicing with the right.

The subject C. seemed to give closer attention to the copy when practicing with the left hand but more especially at the beginning of eachexperiment. As was the case with most of the others, the principal consideration at the beginning of each experiment seemed to be to make the circle the same size as that of the copy; but when the copy did not correspond to the natural adjustments of the muscles and when an attempt to follow the size of the copy caused an increase of the error, the subject's æsthetic feeling for smoothness of contour in his own curves soon gained control of the movements, and consequently the size of the copy was wholly dis-regarded.

Since S. F. made such small gains in the decrease of the angularities of her curves, we are justified in concluding that she directed her attention more especially to making her curves the same in size as the copy. Though there was a constant tendency to increase the size of successive circles throughout the experiment, yet the table shows that she decreased the average size of the circles in six days' practice with the right hand from 70^{mm} to 60^{mm}, and with the left from 70^{mm} to 58^{mm}. I observed that during the first experiment she paid more attention to the smoothness of her curves than to their size, but with practice, however, she became more regardful of their size. In order to make a test on the point whether the size of the copy had any influence on the error in the drawings, I made a few experiments on this subject in which no copy was used. As a result the circles were somewhat larger than those made at

the first experiment and the error was less. These results for S. F. furnish an example of the development of the inhibitory powers in overcoming the muscular adjustments for larger movements of the arm.

The most important feature of practice with J. F. was the approach in the size of the circles to that of the copy. Questioning the subject at the close of the series brought out the fact that she had fixed her attention more especially on the size of the circle. Her statement is confirmed by the large number of alternations in successive circles, so numerous in fact, that there were scarcely two successive circles of the same size. There was, moreover, no tendency to increase or decrease the size of successive circles as with the other subjects. The correcting process was continued throughout the series but the corrections decreased in amount as practice continued day by day. For instance, there was a variation of 8 ^{m} in the size of the circles drawn with the right hand on the first day while there was only 3^{mm} variation on the last day. Such a gradual reduction of the variations in the size of successive circles from day to day demonstrates the influence of practice in giving control over muscular adjustments. The exaggerated adjustments are not in my opinion so much due to the lack of judgment as to the inertia of the muscles; when they do yield to the will, such momentum is acquired that the movement is as likely as not to be exaggerated. The great difference in the development of control over muscular adjustments between J. F. and the other subjects was due to the close attention that she gave to the copy before attempting her circle. She not only observed the copy carefully at the beginning of the exercise but also before attempting each circle.

Daily decrease of error expressed in percentages.

The percentages given in Table VIII. were computed according to the method given on page 61; they show the comparative effects of practice on the different subjects. We notice that equal amounts of practice had influences on the different subjects ranging in value from — 12 % to + 37 %. In the case of B. the average gain was 8 % for the right hand and 13 % for the left. This signifies that although losses occurred on some days yet upon the whole, the good effects of practice overbalanced the evil effects of practice.

The percentages for M.C. show a decrease from day to day with each hand. It is particularly noticeable in the case of the left hand where the gain of the last over the first day amounted to 50 %, which, however, decreased very rapidly until the sixth day when there was no gain over the preceding day. These percentages show that practice had 3 % greater effect in the development of control over the movements of the

left hand than for the right. On the contrary, with C. the gain was 12 % larger for the right hand than for the left. Moreover, the percentages from day to day showed no diminution in the influence of practice.

TABLE VIII.

[illegible] of error due to practice.

Subject.	Hand used.	Serial number of day. 1	2	3	4	5	Ave.
F.	R	0.18	—0.11	0.22	0.23	—0.12	0.08
	L	.60	.31	.14	— .30	— .32	.13
M. C.	R	.32	.31	.14	.21	.17	.23
	L	.50	.42	.24	.17	.00	.27
C.	R	.27	.33	.27	.12	.25	.25
	L	.16	.08	.11	.17	.12	.13
J. F.	R	.30	.37	.33	.50	.33	.37
	L	.29	.27	.26	.25	.50	.32
S. F.	R	.17	.21	.28	.20	.00	.17
	L	.31	.25	.13	.20	.00	.18
H.	R	.18	.22	.17	.07	.14	.15
	L	— .23	— .15	.10	— .19	— .11	— .12
J.	R	.58	.29	.20	.30	.25	.32
	L	— .02	.36	— .04	.46	.14	.18

The percentages given in this table were computed from the results given in Table V. according to the formulas explained on page 61. The values in columns 1, 2, ..., 5, are calculated by formula (2), those in the last column by (3).

The very considerable effect of practice on J. F. showed itself both in the increase of the percentage from day to day and in her making a larger average percentage of gain with each hand than any of the other subjects. As already observed, S. F. varied from the copy more with the left hand than with the right, the reduction of the error being 1 % greater for the left hand than for the right. Considering the fact that she was decidedly right-handed, this is important as it shows how a change in the fixation of attention alters the influence of practice. On the other hand, with H. we find that practice had an opposite effect, for with the right hand there was a gain of 15 %, while with the left there was a loss of 12 %.

Error for successive circles expressed in percentages.

In the same manner as in the preceding section, the percentage of decrease in the error for successive circles was determined, which showed in every case a gain with the right hand during each practice period, ranging in values from 1 % to 27 %. With the left hand, the percentages varied from — 20 % to 21 %, though in four out of seven cases,

the percentages showed a loss for the left hand. This demonstrates very clearly that with untrained muscles a constant standard can usually be sustained only for a very short interval of time. However, the ability for continuing the exercise with increased regularity for a longer time increases with practice.

TABLE IX.

Relative average decrease of error for successive circles.

Subject.	Hand used.	Serial number of circle. 1	2	3	4	Ave.
B.	*R*	14	7	—6	—11	1
	L	6	—17	11	—29	—20
M. C.	*R*	22	17	6	0	11
	L	4	—17	—15	—14	—10
C.	*R*	21	7	10	20	19
	L	17	11	17	12	14
J. F.	*R*	29	20	30	25	26
	L	14	18	12	0	11
S. F.	*R*	21	27	20	40	27
	L	28	22	33	0	21
H.	*R*	19	9	14	0	10
	L	6	—8	—30	—11	—12
J.	*R*	25	17	25	20	22
	L	—54	—4	10	—25	—18

The percentages given in this table were computed from the results given in Table VI. according to the formulas on page 61.

Observations and deductions.

I observed that when a special effort was made it was usually accompanied by unnecessary movements of the body. For instance, C. in the first part of the series of experiments would contract the jaw muscles; J. would bite his lips; some would twist the mouth; others would knit the eyebrows. As practice continued, however, and the action became more habitual, these distortions for the most part disappeared. Undoubtedly whenever there is a tension of some muscles while others are being vigorously exercised, they become influenced in proportion to this tension.

In some instances I have observed that when the subject noticed an irregularity in his figure, a desire to improve upon this seemed to excite the nervous centers so much that the following effort would not be so good as the preceding. Although the gain in proficiency is not entirely a physiological process, yet any chain of actions must be repeated a number of times before it becomes established in the automatic centers. However, the amount of practice required for any chain of actions to be

carried on with the minimum amount of attention and effort is a psychological factor, depending upon the intellectual vigor of the individual.

These experiments bring out some striking differences in the development of the movements of the right and left hands. For instance, the practice curve for the right hand followed closely the direction of the habit curve; while with the left, instead of there being a gain during each practice, there was, in some instances, a decrease in accuracy of the drawings as the practice continued. This difference in the practice curves for each hand, I attribute to the special effort of attention called forth at the beginning of the experiment with practice of the left hand. When practice is carried on until the movements become irregular, the practice becomes injurious, for the irregular movements become incorporated into the chain of reactions as certainly as do those which are purposefully directed. Therefore, practice may tend to establish irregular adjustments as well as regular ones. Speaking figuratively, the capital on hand at the beginning of each succeeding practice period is the sum of the preceding practices. Consequently, the larger the probable error of the average of all the preceding practices, the more irregular will be the movements of the muscles at the succeeding practice. Hence better results might have been obtained in those cases where the error increased after the third circle if the practice periods had been shorter in the first part of the series and more prolonged in the latter part.

Owing to the fact that some of the subjects increased the size of successive circles and others decreased them, we are justified in concluding that there is a certain adjustment of the muscles in writing and drawing most suitable for each individual which should be taken into consideration when training the muscles for accurate adjustment. If the size of the copy corresponds to the natural adjustment, the subject needs only to direct his attention to the smoothness of his figures, otherwise, he has to contend with the distracting element of the size. Therefore the amount of gain in accuracy of adjustment will be influenced thereby. The general conclusion is that, in the earlier stages of muscular development, the size of the copy should be adjusted to the natural movements of the muscles. If this is not done it may prove such a distracting element that the subject will discard it altogether, for, as we have seen, the attention is always directed first toward smoothness of contour, or freedom from angularities. Psychologically the order of development is in such movements as writing and drawing, (1) reduction of irregularities, (2) correctness in size; and in movements where agility is involved, (1) rapidity, (2) regularity.

The correspondence in the decrease or increase of the size of the

circle and the average daily error, as shown in these experiments, indicates (1) that some distinct relation exists between the error and the size of the circle; (2) that the subject's attention, especially in the case of the right hand, was usually directed to the decrease of the error rather than to making the circles of a size corresponding to that of the copy; (3) that the muscles of the right hand, trained to make certain movements, found it difficult in some instances to establish an entirely new set of reflexes.

Finally, these results support the principle that a short exercise often repeated is the best method of practice for rapid development of accurate adjustments of the muscles. There is no doubt that many of the long exercises in writing and drawing and other subjects in the school-room often engender habitual inattention in the pupils. They are often compelled to write continuously for several minutes; the wisdom of this is doubtful when we consider that in the case of well-developed persons five trials at one time in the experiments with circles gave the best general results attainable at one sitting. Hence long practice at writing, drawing, piano-playing, etc., seems to be time and energy wasted, for not only are inattentive habits cultivated, but every wrong adjustment of the muscles gains a place in the chain of subconscious memories and therefore delays the development of the control over the muscles for accurate adjustments. The practice at each sitting should last only so long as the movements are purposefully directed.

III. Development of control over untrained muscles.

The object of this set of experiments was to ascertain the influence of practice on entirely untrained muscles and less adaptable joints. The experiments, made on the left large toe of Mr. Davis, covered a period of ten days. Four phases of the toe's movement were recorded, the time of the downward motion, the downward rest, the upward motion, and finally the upward rest. As each movement of the digit required the exercise of several muscles, the object of measuring each of these four phases of the toe's movement was to show: (1) the differences in time for the phases of the movement; (2) the influence of practice on the phases; (3) the influence of practice on the shortening of the time of the entire movement of the toe.

Apparatus.

The apparatus was virtually the same as that previously described in these Studies.[1] The single 100 v. d. fork was, however, replaced by the

[1] Scripture, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 113.

Pfeil and Deprez markers from which wires led into the quiet room[1] and were there connected with a double-contact key. The markers wrote directly on the smoked surface of the drum; the points were placed parallel to each other and in a line tangent to the surface of the drum. When the drum was revolved, the distance between the marks caused by movements of the armatures could be measured with great accuracy.

The key knob moved through a distance of 5^{mm}. At the beginning of the downward movement of the toe the back contact was broken; at the end of the downward movement the front contact was made; at the beginning of the upward movement, the front contact was broken; at the end of the upward movement the back contact was made. Thus the limits were marked for the four phases, downward movement, lower rest, upward movement, upward rest. The latent times were compensated.

Daily averages of the tapping time.

The average tapping times for successive days are given in Table X. The series of taps during each experiment was divided up into three parts. The first section in Table X. includes the averages of the first 30 taps; the second, the 31st to the 50th tap; and the third, the 51st to the close of the experiment.

The results show an almost constant increase in speed from day to day. Beginning with an average of 436^{σ} on the first day, the average tapping time became 212^{σ} after ten days' practice. Only on one day was there a loss and then the subject was indisposed.

Fatigue was always present after about the 50th tap, appearing sooner on some days than on others, as indicated by the probable errors in section C. of Table X.

Practice generally has its greatest effect between the 30th and 51st taps. The first 30 taps formed the training period of the muscles in which the tapping was constantly accelerated. When the exercise was continued until the muscles were partially overcome by fatigue, the tapping became slower and much more irregular. The portion of the experiment lying between the 30th and 51st taps was chosen arbitrarily as a uniform measure for comparison. The most regular part of the day's practice corresponded closely to that marked off in these boundaries. The tendency was, however, for this zone of regularity to move each day slightly farther away from the beginning.

Of the different phases of the toe's movement, we notice that the movement upward was longer than the downward one; likewise the upward rest was longer than the downward rest. The tension of the spring

[1] See above, p. 52.

of the key, although made too slight to be considered, may have also contributed to make the downward rest shorter. That the touch stimulus was the main factor appears likely because by practice the upward rest tended to decrease more rapidly than the downward rest.

TABLE X.

Daily average of tapping times.

Date,	Movement downward.						Downward rest.						Movement upward.					
1898.	A	p.e.	B	p.e.	C	p.e.	A	p.e.	B	p.e.	C	p.e.	A	p.e.	B	p.e.	C	p.e.
XII–14	45	14	39	6	47	12	56	13	97	40	62	20	88	38	84	40	59	18
" 15	31	3	23	7	45	14	63	16	57	14	116	41	40	12	27	9	61	18
" 16	31	5	41	11	42	12	87	41	69	28	77	33	40	9	38	9	41	8
" 17	30	5	32	5	41	12	56	10	56	15	71	17	31	9	31	5	44	12
" 18	29	6	35	10	32	7	86	33	80	25	107	40	30	6	33	4	43	19
" 19	34	9	32	5	34	8	57	13	66	21	63	19	37	10	36	4	46	14
" 20	46	13	58	21	45	11	66	19	70	13	77	16	41	13	38	14	48	10
" 21	29	3	30	3	37	9	73	25	69	9	77	25	27	3	28	3	35	6
" 22	36	10	33	8	37	9	74	19	75	10	83	17	36	9	26	4	30	8
" 23	29	3	31	6	26	4	64	26	74	18	75	13	24	3	25	4	25	4

Upward rest.						Entire movement.		
A	p.e.	B	p.e.	C	p.e.	Tap-time	p.e. in σ	p.e. as %
267	94	272	98	231	70	436	103	24
153	56	169	64	175	67	322	73	23
138	60	98	40	147	69	296	74	25
171	99	150	62	135	52	284	68	24
112	44	102	45	134	54	281	61	22
109	49	138	58	153	65	273	62	23
157	62	97	31	109	45	275	68	25
105	43	93	37	120	53	241	54	22
81	22	76	25	83	21	221	36	16
99	29	73	22	71	18	212	35	17

Unit of measurement, $1\sigma = 0.001^s$.
The probable error of a determination varies from $\pm 23^\sigma$ to $\pm 0.5^\sigma$.
A, the daily average of the first 30 taps.
B, " " " " " following 20 taps.
C, " " " " " remainder.
p.e., probable error.

The most noticeable effect of practice consisted in the change of the probable error of the upward rest, which decreased after ten days' practice from 94^σ to 29^σ for the first 30 taps; for next 20 taps from 98^σ to 22^σ; and after first 50 taps, from 70^σ to 18^σ. Likewise, the movement upward shows a greater gain in regularity than the movement downward; it decreased in section A., from 38^σ to 3^σ; in section B., from 40^σ to 4^σ; and in section C., from 18^σ to 4^σ. The conclusion to be drawn

from the disparity in the amount of decrease of the probable error for the different phases of the toe's movement is that, since in the movement upward and in the upward rest there was a greater amount of voluntary

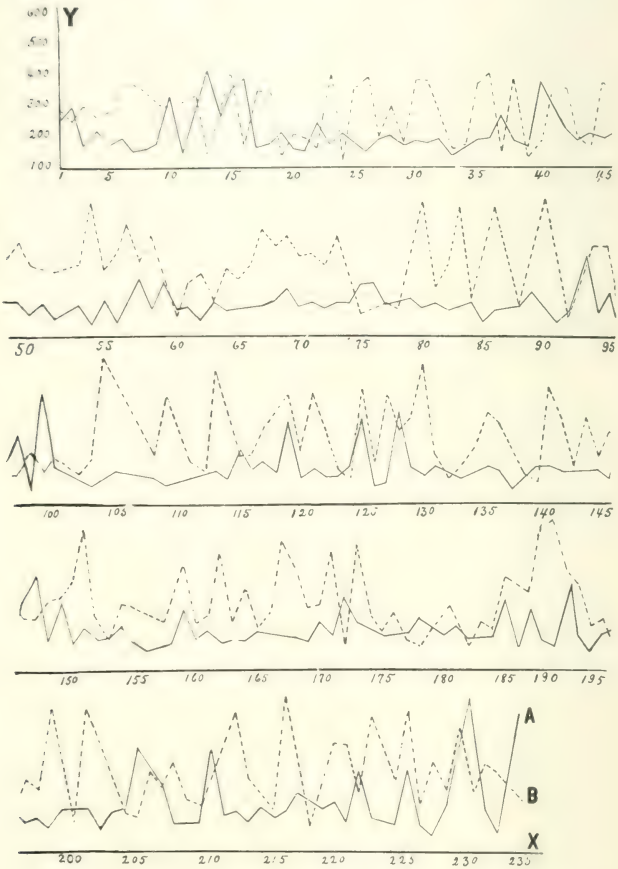

FIG. 10.

X, serial number of tap.

Y, tap time in thousandths of a second

effort, they were more irregular in the beginning; and as the movement came to be controlled by the automatic centers, the influence of practice was more manifest in these two phases of the toe's movement. Consequently we conclude that in beating time that phase of the movement which causes the greatest irregularity consists in the change of the motion upward to the motion downward.

The daily average tapping time, Table X., shows a decrease of the probable error from 103^σ to 35^σ. Moreover, when the error is expressed as a percentage of the tapping time, it shows that the increase in regularity was larger than the gain in speed, for the relative gain decreased from 24% to 17%.

The average daily increase in speed, according to the formula given on page 61, was determined, with the following results: 169, 62, 40, 33, 37, 39, 50, 25, 9^σ. This expressed as percentages gives: 39, 19, 14, 12, 13, 14, 18, 10, 5%. These figures show that the first day's practice resulted in the largest gain, which, however, rapidly decreased as the practice was continued from day to day. They moreover show that in simple movements, as in tapping, the effect of practice is greater in proportion to the undeveloped state of the muscles.

Physiological effects of practice.

The differences in the fluctuations in the curves of Fig. 10 show the influence of practice. The A curve is the practice curve of the last experiment; B, that of the second experiment.

These curves show how the periods of slight paralysis varied during the two experiments. In curve B the variations follow each other in rapid succession. In curve A they are not so frequent and the recovery is much more rapid. The fluctuations, though larger throughout the series of curve B, increase in amplitude and frequency toward the close of the experiment. In curve A the fluctuations are not so pronounced in the second half even as in the first half. Hence the most prominent physiological effect of practice is to delay the arrival of entire paralysis of the muscles and the reduction of fluctuations in the practice curve. This depends, however, to a large extent on the nervous condition of the subject, for on some of the intervening days absolute paralysis of the muscles occurred before the 125th tap.

At the beginning of the series of experiments intense pain was felt in the calf of the leg after a short period of practice. As exercise continued, the pain became sympathetically induced also in the calf of the other leg.

The coming on of fatigue was characterized by the following stages: (1) a feeling of strain throughout the tendon above the heel; (2) slight

pain in the calf of the left, or exercised, leg; (3) pain was felt in the muscles controlling the upward flexion of the toe; (4) paralysis gradually spread over the whole of that leg while the pain continued to increase in the above mentioned muscles; (5) the pain was finally felt in the calf of the right leg. On the fourth day the pain did not recur in the calf of the leg but was felt in the tendon immediately over the second joint of the toe; but before the close of the experiments it had ceased altogether.

Likewise, in a series of dumbbell exercises wherein I acted as subject, on first to fourth days inclusive the pain was so intense that I was able to exercise only a short time, but after the fourth day I was able to raise one dumbbell over 1000 times without experiencing any actual pain, the only noticeable effect being considerable fatigue.

Throughout all the experiments, I noticed that on some days there was a tendency to decrease the speed from the very start, while on others there was at first a period of acceleration for a limited time before the decrease began. I consequently performed some experiments upon myself in order to determine whether this was connected with the rate of the heart-beat. The experiments were made in tapping with the toe and in the exercises with the dumbbells. Although the tests were not entirely conclusive yet in a general way I found that there was at least some relation between them. When the movements were as rapid as possible there was a decrease in the heart's action. I also observed that when the nervous system was in a relaxed state there was a rapid increase in the pulse beat in the early part of the exercise, the pulse becoming full and firm. Accompanying this there was an increase in the rate of voluntary activity.

The fluctuations in the practice curves may be due to the following causes: (1) fluctuations of the attention caused simply by the lack of effort on the part of the subject; (2) local paralysis of the centers governing the muscles brought on by exercise; (3) mental paralytic strokes causing fluctuations which are generally ascribed to mere fluctuations of attention; (4) general physiological fatigue of the whole body; (5) general mental fatigue.

The above conditions are ever changing, for the physiological effects of practice and exercise depend upon the supply of energizing material stored up in the muscular tissue, the supply of oxygen to the blood, temperature, etc. The mental condition is, moreover, more or less dependent on the physiological condition.

General mental fatigue was distinguished from general muscular fatigue by the feeling that resulted after a period of violent exercise. The muscles of the subject sometimes became fatigued almost to the de-

gree of paralysis, but no special mental effects were felt. On one day after the subject had been engaged with some difficult computations, the change of exercise seemed at first to be a relief, but in a few seconds it became quite difficult for him to control his attention. Though the rate of tapping on that day was as rapid as on any preceding day, yet the mental fatigue was much greater.

A distinction can be made between mental and muscular fatigue by the fact that the coming on of mental fatigue is spasmodic and irregular. An aberration of the attention marks the initial stage of mental fatigue. This soon passes over into a stage which materially effects the regularity of the muscular action.[1] No such irregularities characterize muscular fatigue which is governed by physical laws controlling the breaking down of muscular tissue and the dissemination of waste products.[2] Mental fatigue, however, is subject to the effects resulting from fixation of the attention and thus partakes of all its fluctuations and irregularities.

Effects of practice on muscular action.

1. Practice causes increased circulation, furnishing thereby a large supply of food materials to the muscles, and more rapid dissemination of waste products. Thus the muscles are able to store up energy and give it out more readily on demand. But the storing of energy is not all. For example, gymnastic exercise has a higher purpose in view than to bring the body merely to that state of perfection found in the case of the common laborer. The development of muscular tissue must be supplemented by an education of the nervous centers in order that they may respond precisely to acts of will. Indeed, the increase of muscular tissue may be looked upon as merely an accidental accompaniment to that mental process which begins by constant fixation of the attention and ends, even where the achievement is most complicated, in automatic and subconscious control of the muscular movements.

2. The time before the coming on of mental and muscular fatigue depends upon the amount of muscular energy and upon the concentration of the attention, both of which are greatly influenced by practice.

3. When for any set of actions the development of the centers has reached an automatic condition, the maximum rate of either mental or muscular rapidity is not reached in the first few seconds of the exercise because a chain of actions cannot be remembered by any act of will but requires the exercise of the muscles themselves to reëstablish the chain of subconscious reflexes.

[1] MOORE, *Studies of fatigue*, Stud. Yale Psych. Lab., .1895 III 89.

[2] LOMBARD, in the American Text-book of Physiology, III, Philadelphia 1897.

4. The duration of maximum rapidity is dependent upon the power of the mind for sustained attention. As soon as the attention is diverted the movement comes into the control of the automatic centers. The speed is consequently decreased, for only by a special effort is speed, either muscular or mental, increased above the limit acquired by habit. The utmost speed can be maintained only for a few seconds at a time at first, but the period may be lengthened by practice. Hence the fluctuations in the practice curve are generally due to slight mental fatigue. Although the subject recovers very rapidly at first, yet as the exercise continues the fluctuations recur more frequently and the periods of recovery are lengthened.

5. Another very important element connected with the duration of one's ability for continuing the exercise, is his knowledge of the time that the exercise is to last. If the exercise is to last only a short time, greater effort will be put forth in that period than when the exercise is to continue an hour or more. In long periods of exercise the subject will unconsciously measure out the energy in proportion to the duration of the practice period. For instance, in the experiments made by OEHRN[1] in memorizing syllables, in making successive additions, and in counting letters in groups of threes wherein the exercise was continued from one to two hours, the maximum was reached only in the first instance after 24 minutes, in the second after 28, and in the third after 59. Contrasted with our experiments, his results show that the maximum point depends upon the effort put forth in the beginning of the experiment. The results that I obtained point to the fact that if OEHRN had shortened the practice period, the maximum point would not only have been reached in about one tenth of the time, but the progress no doubt would have been greater.

Nor does the maximum rate of voluntary movement depend upon an innate sense of rhythm as SCHAEFER[2] implies but is, as CAMERER[3] states, one of constant acceleration until the setting in of mental fatigue. Instead of a rhythmical fluctuation in the voluntary effort, maintained by some investigators, I found no remarkable regularity and am led to consider it as dependent upon several psycho-physiological processes too complicated to have a regular period of oscillation.

6. Practice does not always mean an absolute gain in efficiency ; it may

[1] OEHRN, *Experimentelle Studien zur Individualpsychologie*, Psychologische Arbeiten (Kraepelin), 1896 I 92.

[2] SCHAEFER, CANNEY AND TUNSTALL, *On the rhythm of muscular response to volitional impulses in man*, Jour. Physiol., 1886 VII 96.

[3] CAMERER, *Versuche über den zeitlichen Verlauf d. Willensbewegung*, 41–45, Tübingen 1866.

even produce negative results. It is generally taken to signify improvement; but observation and the results of experiments discussed in preceding pages, show that, when either muscular rapidity or regulated movement is required, practice may be even detrimental to development. Every action leaves its trace on the nervous matter; every effort put forth tends to establish itself so that acts immediately succeeding it follow as a matter of inertia. If the effort put forth is small and the action slow, habit then establishes itself for that mode of action. The law of the growth into habitual automatic control takes into account every activity. If a higher speed of activity or a regulated movement or a certain readiness in mental grasp is desired, then every thoughtless action tends to establish itself and delays attainment to the desired standard of efficiency.

It requires the same effort to overcome the condition occasioned by bad effects of practice as it does to establish a new mode of functioning. Hence energy is wasted when the practice is not thoughtfully directed. Therefore, we conclude that not only is that part of practice efficient for growth in regulated movement, in speed of muscular adjustment, or in quickness of mental grasp, which is accompanied by conscious effort, but the unconscious adjustments also have their effects and should be directed properly.

7. The feeling of satisfaction or of having attained one's limit is another not less important element in the development of rapidity in either mental or muscular activity. Every advancement either in mental quickness[1] or muscular activity requires a certain effort, depending on the stage of development already attained. The greater the speed and the smaller the probable error, the less the gain becomes for the same expenditure of energy. As this developed state is approached, a person feels that his efforts are not sufficiently rewarded, and finally there comes a period when he feels that he has reached the limit of his development. In fact, this constitutes the difference between the novice and the expert. The "plateaus" mentioned by BRYAN[2] in the habit curve would seem rather to indicate resting periods in the effort. If the subject can be induced to sustain the same effort day by day, there would not be any "plateaus" in the habit curve.

If this law be expressed by the general equation $y = f(x)$, where y indicates the amount of gain by practice, we must regard x as containing constant elements of (1) time, (2) complexity of

[1] AMBERG, *Ueber den Einfluss von Arbeitspausen auf die geistige Leistungsfähigkeit*, Psychologische Arbeiten (Kraepelin), 1896 I 30.

[2] BRYAN, *Studies in the physiology and psychology of telegraphic language*, Psych. Review, 1897 IV 27.

the movements, (3) the number of muscles undergoing training, and (4) the growth of automatic control. The last may be resolved into various personal factors such as mental grasp, endurance for sustained effort, and the vividness of the impression. According to this principle, then, the growth of intellectual habits should be more rapid with those who possess the strongest intellectual powers, since, by their power of holding the attention, they succeed in getting the same impression repeated oftener in the chain of reactions. If with intellectual power there is combined strong individuality, or perhaps more properly speaking a strong will, such persons possess superior ability after breaking down one habit and reforming another.

IV. Estimation of time.

The apparatus consisted of a kymograph, to which was attached the Wundt time-sense apparatus with the Meumann star-contacts by means of which adjustments could be made so as to give any interval of time desired. The arrangement of the apparatus was the same as that described in these Studies.[1] The sound of the 100 v. d. fork was conveyed to the subject in the quiet room by means of a telephone receiver. The sound to be estimated came first. After an instantaneous interruption the sound began again. When the subject thought that it had lasted as long after the interruption as before it, he pressed a key which recorded a spark on the drum of the kymograph. The contacts were so

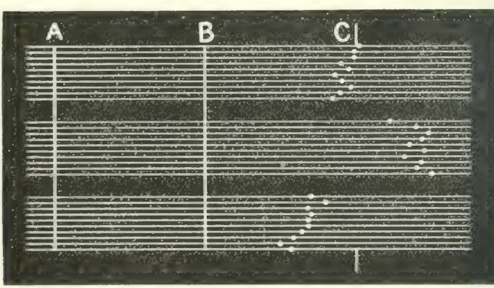

Fig. 11.

arranged that an experiment occupied the first part of every period of 18^s, the remainder of the period serving for rest. This was intended to avoid as far as possible any influence of fatigue. A series of such records is given

[1] Scripture, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 127.

in Fig. 11. *A* represents the beginning of the tone; *B*, the point where it was interrupted; and *C*, the point where the interval of time from *B* to *C* was equal to that from *A* to *B*. The dots represent the first ten estimates of 100^{σ} by the subjects A.F., R.E. and H.J. on the first day; the group at the top of the figure being the estimates for A.F., the second those for R.E. and that at the bottom those for H.J. This may be said to represent in a general way the ability of different individuals to judge intervals of time without practice.

A number of preliminary experiments demonstrated the fact already established by a number of investigators that the time-judgment varied considerably for the same interval with different individuals and with the same individual for different intervals. Attempts have been made to establish a definite interval as that which can be estimated with the smallest amount of error, but the point has varied with each investigator.

The three intervals, 82^{σ}, 100^{σ} and 164^{σ}, were selected because they covered the field of most accurate estimates according to the results obtained by previous investigators.

Those who served as subjects for the experiments were W.J. (Jump), R.E. (Evans), E.F. (Furguson) and B.B. (Brown), students in the Theological Seminary; A.F. (Fisher) and C.S. (Smith), steward and mechanic of the Psychological Laboratory.

Daily average estimates.

The average estimates for successive days are given in Table XI., and are graphically represented in Figs. 12, 13 and 14.

Every precaution was taken to prevent the subjects from counting or moving any part of the body by which they might measure off the time through muscular energy, the object being to ascertain whether a person has an actual time-sense regardless of any form of muscular activity or mental calculation.

An inspection of the results for A. F. shows that there was a constant decrease in the time estimate from day to day. Beginning with 88^{σ} on the first day, he gradually reduced the estimate during 16 days' practice to 55^{σ}, which is but little more than one-half of the time to be estimated. The same facts were brought out in the case of R. E., whose average estimate of 100^{σ} was on the first day 159^{σ}, and 102^{σ} on the eighth day. This shows a decrease of about one-half in the average estimate. H. J. increased his average for the first day, 99^{σ}, until the third day when he reached his maximum point, 116^{σ}, after which the estimate decreased until next to the last day. The very large estimate, 137^{σ}, on the last day, no doubt, was due to nervousness. The record for this day should not be

TABLE XI

[illegible]

	A. F.				R. E.			H. J.		
Date 1898.	Estimate on 100^{Σ}	p. e.	p. e. as %	Date 1898.	Estimate on 100^{Σ}	p. e.	p. e. as %	Estimate on 100^{Σ}	p. e.	p. e. as %
XI–26	88	1.4	1.7	XII–15	159	1.5	0.9	99	1.4	1.4
27	91	1.7	1.9	16	130	1.6	1.2	103	1.6	1.5
28	73	1.1	1.5	17	138	1.8	1.3	116	1.1	1.0
30	88	1.4	1.7	18	118	1.3	1.1	114	1.1	1.0
XII–1	74	1.6	2.2	19	114	1.1	1.0	114	1.5	1.3
2	73	1.5	2.0	20	117	1.1	1.0	109	1.2	1.1
3	58	1.3	2.2	21	107	0.9	0.8	103	1.0	1.0
4	54	1.7	3.1	22	102	0.8	0.8	137	2.3	1.7
5	53	1.2	2.3							
6	58	0.8	1.4							
7	61	1.0	1.6							
8	53	1.3	2.5							
9	55	0.8	1.5							
10	58	1.1	1.9							
11	55	1.0	1.8							
12	55	1.2	2.2							

	B. B.			E. F.			A. F.			C. S.		
Date 1898.	Estimate on 82^{Σ}	p. e.	p. e. as %	Estimate on 82^{Σ}	p. e.	p. e. as %	Estimate on 164^{Σ}	p. e.	p. e. as %	Estimate on 164^{Σ}	p. e.	p. e. as %
I–26	97	1.1	1.1	73	1.5	2.0	91	6.5	7.1	147	3.3	2.2
27	99	1.1	1.1	70	1.0	1.4	99	2.3	2.3	157	2.0	1.3
28	102	1.8	1.8	69	0.9	1.4	105	2.5	2.4	164	2.6	1.6
29	113	1.7	1.5	82	1.5	1.8	119	2.2	1.8	162	2.1	1.3
31	112	1.6	1.4	80	1.1	1.4	118	2.5	2.1	167	2.2	1.3
II– 1	113	1.5	1.3	82	0.9	1.1	157	2.6	1.6	166	3.9	2.3
2	115	0.8	0.7	80	1.5	1.9	154	2.4	1.6	180	2.5	1.3
3	107	1.5	1.4	92	1.8	2.0	155	3.4	2.2	207	2.8	1.4
4	101	1.3	1.3	99	1.2	1.2	169	2.8	1.6	175	2.7	1.5
5	116	1.3	1.1	99	1.4	1.4	150	3.1	2.1	205	2.7	1.3
6	118	1.5	1.3	87	1.8	2.1	159	3.5	2.2	199	2.4	1.3

Unit of measurement, $1^{\Sigma} = 0.01^{s}$.

Number of estimates on each day, 30 to 50.

The probable error of a determination varies from $\pm 10^{\sigma}$ to $\pm 2^{\sigma}$.

p. e., probable error of each estimate from the average.

considered in making up our deductions to be drawn from the results, for the subject was in a very nervous state when the experiment was made. I included the record in the table only to contrast the difference between the nervous and tranquil frame of mind in respect to the estimate of time.

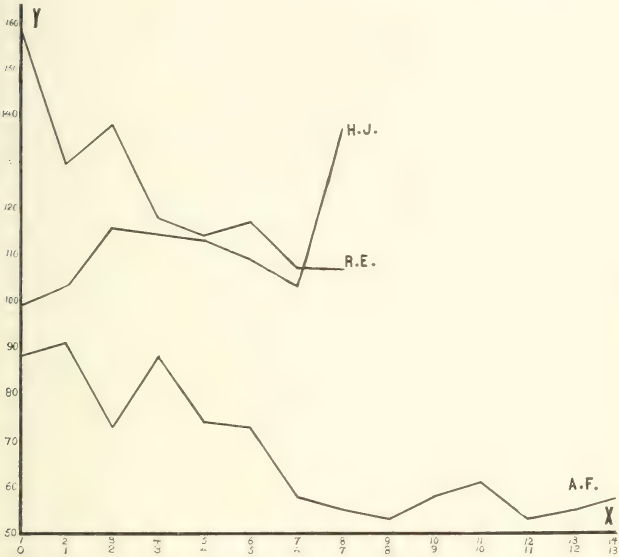

FIG. 12.

X, upper line, serial number of day.
X, lower line, days of previous practice.
Y, estimate of an interval of 100^{Σ}.

In the cases of B. B. and E. F., Table XI., where 82^{Σ} was the interval given, we notice that with each of these subjects, the time estimate was continually lengthened as practice continued. B. B. began with an average estimate of 97^{Σ} on the first day, which was larger than the interval to be estimated and continued to increase the interval throughout the series of experiments. E. F. followed the same course, but he began with an average estimate of 73^{Σ} on the first day, which increased daily, until his estimate exceeded the correct amount by 15^{Σ}. Hence there was with

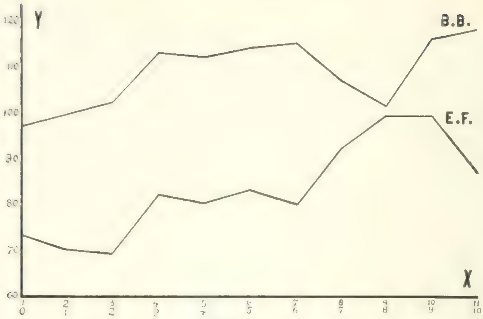

FIG. 13.

X, upper line, serial number of day.
X, lower line, days of previous practice.
Y, estimate of an interval of 82Σ.

FIG. 14.

X, upper line, serial number of day.
X, lower line, days of previous practice.
Y, estimate of an interval of 164Σ.

E. F. a total increase of 24^{σ} in his estimate, and with B. B. 21^{σ}. These amounts divided by the number of days (11), would give an increase of over 2^{σ} per day. Likewise A. F. decreased his average daily estimate by 33^{σ} in 16 days, a daily decrease of 2^{σ} per day. R. E. decreased his estimate 57^{σ} in eight days, a decrease of 7^{σ} per day. The correcting process present in the case of H. J., no doubt, caused the irregularities in his habit curve, for the subject was aware of the tendency to underestimate long intervals and to overestimate short intervals. Consequently he continually corrected himself. But even in his case, we notice that after the third day's practice there was a tendency to shorten the interval.

When 164^{σ} was estimated, the same facts were found to exist. However, it should be noted that where A. F. decreased his estimate when 100^{σ} was the given interval, he increased it from 91^{σ} to 159^{σ} (the highest point being 169^{σ}, 9th day) in estimating the 164^{σ} interval; C. S., likewise, increased his estimate of 164^{σ}, approximately 4^{σ} per day.

When the 100^{σ} interval was estimated, all may be said to have decreased their estimates of its duration by practice, varying in amount with each individual. Moreover, each had a different conception of its duration in the very beginning of the experiments, ranging from 88^{σ} to 159^{σ}. When 82^{σ} and 164^{σ} were the intervals estimated all the subjects increased their estimates of it by practice. The estimates of 82^{σ} on the first day were 97^{σ} and 73^{σ}. Of 164^{σ}, the estimates were 91^{σ} and 147^{σ}.

In Table XI. are given the probable errors for each day's estimate. In the third column the probable error is expressed as a percentage of the estimate. Though there was a decrease in the absolute probable error with each of the subjects, yet when it is expressed as a percentage there was only one case, C. S., in which the relative probable error decreased more rapidly than the average estimate decreased or increased as the case may be from day to day. This fact demonstrates that the changes in the daily average estimates were not due to fatigue. If the effects of fatigue had been more prominent in the early part of the series than in the latter part then there would have been a decrease in the percentage of error. But there was little cause for fatigue, since the subject was required to make only one judgment every 18 seconds.

The facts brought out in these experiments are important as throwing some light on the various "indifference points" obtained by different investigators. The estimate is a personal factor which varies with each person and with the same person at different periods of practice. To make a test of this point, after the the close of the series I changed the interval of 100^{σ} to 75^{σ} and gave A. F. a few tests. The results were: 50, 49, 38, 49, 49, 59, 36, 43, 47, 39, 53, 35, 39, 33, 49, 51, 38, 43, 33,

36 (ave. 43^{Σ}; p. e., 4^{Σ}). These figures, however, may have been influenced by practice on the 100^{Σ}; the large probable error would seem to indicate it.

The above figures, as well as the changes in the time estimate due to practice, Table XI., would seem to indicate that the interval which we judge with maximum accuracy is a changeable one, depending upon the amount of previous practice. Nor does it seem that our "time-sense," when unaided by some form of muscular action, is sharpened by practice; on the other hand it may become less accurate. Without doubt, if some additional basis were allowed, as in counting or moving of the hand, practice would undoubtedly produce good results, in which instance it becomes a measure of muscular strain when carried into overt acts, or of strain of the attention when only the impulse is felt. The differences then in the time estimate of different individuals depend partly upon their nature, whether impetuous and nervous or temperate and deliberate; partly upon the direction of the attention.

While the nervous temperament may account for the difference in time estimate of different individuals, it does not explain the variations in estimate day by day. For instance, in the case of A. F. (the only subject practiced on two different periods) the case is reversed in the two estimates. He not only underestimated the 100^{Σ} at the very beginning of the series of experiments but continued to reduce the estimate throughout the whole series. On the other hand, in estimating 164^{Σ} his underestimate was larger in the beginning of the series than at its close. Consequently it may be inferred that the change in the estimates of the two periods was not due so much to the nervous temperament of the person as to the conception of the length of the interval which he entertained at the beginning of the series of experiments.

Summary.

1. The estimate of a given interval varies for different individuals both with and without practice.

2. The estimate varies for different intervals with the same person both with and without practice.

3. Practice on the same interval may cause the variation from the given interval to increase with one person and decrease with another. Or it may cause the variation from the given interval to increase with one interval and decrease with another interval.

4. Time estimate is a personal factor depending upon (a) the nature of the person whether of an impulsive or quiet temperament; (b) upon the point of the fixation of the attention whether to the sensory or to the

motor side; (c) upon the acuteness of the sense of the person to strains accompanying the fixation of the attention and muscular tension.

5. The change that practice produces in the estimation of time is probably due to fixing the attention in the beginning of the series of experiments: (a) to the movement to be performed, in which case the time estimate is shortened in accordance with the growth of automatic control; (b) upon the sensory side in which case the time estimate is made longer; (c) on the idea which the subject entertains of the interval on beginning the experiment—if he considers it very short, he will at first underestimate it and will increase the time estimate by practice; on the other hand, if he considers it very long he may shorten it by practice.

6. Our sense of time may not be sharpened by practice; on the contrary, it may become less accurate.

7. There is no "indifference point" from which the subject does not vary with long continued practice.

8. The overestimation of small intervals may also be due to the fact that the subject does not take into consideration his reaction time. The underestimation of longer intervals may also be due to the fact that the impulse to react keeps ripening and soon reaches such a degree of intensity that the subject cannot withhold the reaction.

V. Regulated rhythmical action.

Apparatus.

By regulated rhythm we understand such actions as beating with a signal. The sound which guided the subjects in beating time was produced by a telegraph sounder, arranged in series with a make contact on a revolving drum. At a point on one of the upright standards supporting the drum a spring was attached. To the drum a small projecting arm was attached. When this arm moved round to the spring attached to the standard supporting the drum, it pressed a lever down, thus closing the circuit through the telegraph sounder, and thereby producing a click in the sounder. Consequently, the time elapsing between successive clicks of the sounder depended upon the speed at which the drum was revolved; the regularity in the frequency of the sounds depended upon the regularity in the speed of the drum.

The drum was run by a small motor to which it was attached by a thread belt. The motor was run by a current produced by three Edison-Lalande batteries, regulated by introducing a resistance of small German-silver wire. In this way the speed of the motor could be adjusted with

great accuracy.[1] This method of producing sounds at regular intervals required a motor and drum of great regularity. A careful investigation was, therefore, made on several kinds of drums and recording arrangements.

1. EDISON *phonograph.* The metal cylinder of the phonograph may frequently be used as a recording drum. Three tests at different speeds were made on the form known as the Home Phonograph. At 267.5^{mm} per second, the probable error was 1.85^{mm}, or 0.7% ; at a speed of 174.2^{mm} per second, it was 0.42^{mm}, or 0.2% ; at a speed of 354.3^{mm} per second, it was 0.84^{mm}, or 0.2%. The drum is thus a very regular one. The small size of the drum is inconvenient, but the works are so strong that they might be used in running a much larger drum.

2. LUDWIG *kymograph made by* BALTZAR. At the slowest speed, 0.47^{mm} per second, the probable error was 0.008^{mm}, or 2% ; at a speed of 0.9^{mm}, the probable error was 0.01^{mm}, or 1.0% ; at 28.9^{mm}, the probable error was 0.14^{mm}, or 0.2% ; at 254.2^{mm}, probable error was 1.64^{mm}, or 0.6%. I observed that at the slower speeds one is likely to keep the spring wound up too tightly, in which case the probable error is increased. Even one-half turn of the handle which winds the spring will produce considerable variation in its speed.

3. EDISON *motor run by* EDISON-LALANDE *batteries.* Four tests were made at different speeds and on different dates. Three cells, arranged in series, were used to run the motor. A thread from the motor ran the regular recording drum. The first test was made when the cells were fresh. At the high speed of 1037.7^{mm} per second (over two revolutions per second), the probable error was 0.3%.

The next test was made nine days later ; the cells had been in constant use in the meantime. The same arrangements were used, but a slower speed was tried. At 159.3^{mm} per second, the probable error was 0.12%. Ten days later another test was made at a speed of 265.5^{mm} per second, the probable error was 0.08%. In another ten days a test was made at a speed of 150.5^{mm}, the probable error was 0.13%.

These tests show that an EDISON motor run by the EDISON-LALANDE batteries properly arranged, is a very regular source of power. It should be stated that the cells must work for several minutes before they become constant. In no case was I able to get a favorable record until the cells had been working at least five minutes. The above tests were made after the cells had been working ten minutes or more.

[1] The remainder of the apparatus used was identical with that described in SCRIPTURE, *Elementary course in psychological measurements,* Stud. Yale Psych. Lab., 1896 IV 121.

4. *Latent time and regularity of the* Pfeil *marker.*

(1) Break contact. When the break contact was used and the magnetic cores were removed as far as possible from the armature to which the metallic point of the marker was attached, the latent time was found to be 1.1^σ, with a probable error of 0.09^σ. The current was not strong enough to force the armature to react promptly; hence the large probable error. When the cores were placed in close contact with the armature, the remanent magnetism was so strong that it caused the latent time to be considerably lengthened, namely to 14.7^σ; but the probable error was reduced to 0.03^σ.

(2) Make contact. When the cores were distant, the latent time was 1.8^σ, with a probable error of 0.12^σ. With the cores close up to the armature, the latent time was reduced to 1.3^σ, with a probable error of 0.5^σ.

If one desires the smallest probable error possible, then the adjustment with the cores close up to the armature on the make contact, and with them distant on the break contact is the most favorable. Several tests were made on intermediate amplitudes of movement of the armature, with the result that the latent time varied approximately with the distance of the cores from the armature, and the probable error inversely with the distance.

5. *Latent time and regularity of the* Deprez *marker.* The results of the test on the make contact showed the latent time to be 2.5^σ, with a probable error of 0.64^σ. The latent time in the case of the break contact with the same adjustment as above was 3.8^σ, with a probable error of 0.07^σ. Owing to the delicacy of the instrument, changes in the amplitude of the armature did not affect the latent time so much as they did in the case of the Pfeil marker. It did, however, vary somewhat with the strength of the current passed through it, the latent time increasing with the strength of the current.

6. *Regularity of the spark record.* The probable error of spark records under the usual conditions at the Yale Laboratory is 0.25^{mm}. With the same apparatus and the same kind of paper the above amount is constant whether the drum is still or in motion. Consequently the amount that it will vitiate the record depends upon the speed of the drum. With a fast drum it is negligible; with a slow drum it must be taken into consideration. In these records the sharp metal point of the marker was so bent that it stood perpendicular to the surface of the drum. If it is placed at an acute angle to the surface of the drum, the error is largely increased. When thus placed the spark often-times leaps off at the side rather than at the end of the pointer. The thinnest glazed paper is used.

7. *Noiseless key.* For these experiments a noiseless key was necessary because it was found that, while beating time, the subject would quite as often be guided by the sound produced by the key which he used as by the sound coming from the telegraph sounder. After various attempts to get a noiseless key, that which was found best adapted to the purpose of our experiments consisted of a small band of pendulum-wire soldered to a piece of brass which was fastened in a handle. The band rested against a platinum point so that when it was moved the circuit was interrupted. An insulated wire from one pole of the battery was connected with the platinum point through the handle, while the band was connected with the other pole. A soft substance for the finger to strike against in beating time was fastened over the end of the band. By this method we were able to do away with the guidance which the sound from an ordinary telegraph key gave to the ear in directing the beats. Consequently it was necessary for the subject to direct his attention more to the muscular feeling than to the coincidence or the variance between two sounds as would have been the case had a sounding key been used.

The experiments were made on I. M. (Ishiro Miyake), A. F. (A. Fisher), and W. J. (W. Johnson). The time interval was one second.

Experiments.

A number of preliminary tests on different persons showed a uniform tendency to anticipate the signal made by the telegraph sounder with this form of key, whereas with a sounding key the reverse was quite as often

TABLE XII.

Daily averages.

	I. M.		*A. F.*		*W. J.*	
Date 1898. Nov.	Constant error.	Probable error.	Constant error.	Probable error.	Constant error.	Probable error.
14	—118	39	—158	44	—159	36
15	136	35	— 60	37	—148	29
18	— 40	30	— 80	42	—150	22
19	+ 1	66	+ 27	26	— 15	39
20	— 4	29	+ 65	48	— 24	17
21	+ 12	28	— 4	39	— 26	20
22	+ 14	31	+ 10	31	— 37	15

Unit of measurement, $1\sigma = 0.001^s$.

Number of beats at each experiment, 40.

The sign — indicates that the subject beat before the signal was heard; +, that he beat after the signal was heard.

The probable error of a determination varies from $\pm 4^\sigma$ to $\pm 9^\sigma$.

true. A series of such results with a sounding key has been previously published in these studies.[1]

Likewise the results with the noiseless key given in Table XII. show that all the subjects anticipated the signal, not only on the first day, but also during the first three days in the cases of I. M. and A. F., while W. J. anticipated the signal throughout the series of experiments.

In each case the anticipation of the signal decreased until, in the latter part of the series, two of the subjects waited until they heard the signal before beating. The figures given in the table each represent the averages of 40 beats. The constant error was derived by adding all the + and — errors in each experiment, taking their difference and dividing by the number of experiments.[2]

Conclusions.

The probable error for each of the subjects was in the beginning of the series of experiments much less than the constant error. This shows that the tendency of the subject at first was to be governed by the muscular rhythms or his own most natural rate of rhythmical movement. With practice, however, he learned to accommodate the muscular rhythm to the time interval of the signal. However, the diverting of the attention to the signal prevented the rapid decrease of the probable error as was observed in all the other experiments. The psychological order of development in all regulated rhythmical movements is (a) the change from the ordinary rate of muscular action to that of the given rate, and (b) then, the decrease of the probable error.

VI. Free rhythmical action.

The apparatus consisted of a revolving drum on the smoked surface of which the metal point of a Deprez marker wrote. The spark method was used. Wires from a battery of 4 ampères were connected with the mercury cups of a Kronecker interrupter. As the vibrating arm of the interrupter dipped down into the mercury, it closed the circuit through the spark coil. The arm of the interrupter was kept vibrating by a separate self-interrupting circuit through the magnets on either side of the arm. From the secondary coil of the spark coil, one wire led to the drum and the other was connected with the support of the Deprez marker so that at each make of the interrupter there was produced a spark on the line drawn by the point of the marker. The interrupter was adjusted to vibrate 10 times a second.

[1] Scripture, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 123.

[2] Scripture, New Psychology, 182, London 1897.

In order to prevent irregularities in the spark record and to keep the mercury from oxidizing, a stream of water was allowed to flow over the surface of the mercury. According to the original plan two flasks, connected by rubber tubing with the mercury cups, were filled with water and placed above at sufficient height to get the necessary amount of pressure. This, however, was replaced by a much more convenient reservoir, arranged by Dr. Scripture, which was fastened to a tripod and placed a little above the interrupter. The reservoir was connected by rubber tubing with a hydrant. Two rubber tubes were led from the base of the reservoir to the two mercury cups connected with the interrupter. Any amount of water could in this way be supplied. In the event of too large a supply there was a waste pipe connected with the reservoir which prevented it from overflowing.

The DEPREZ marker was connected with a break contact key in the quiet room. Each movement of the key made a break in the circuit, and this in turn produced a movement in the armature of the marker. As in the preceding section, the noiseless key was used because it was desired to ascertain the rhythm of the person when unaided by the ear. The key was held between bags of sand placed on the lap. No restrictions were placed on the amplitude of the movement of the finger or hand in beating time. The subject was directed to choose his own rate of movement and was requested to retain the same speed throughout the series of experiments. All explanations were made before the beginning of the experiments. The subjects were not informed of their rate of movement. In most instances the watch which the subject carried in his pocket was laid aside because it was found that the ticking of the watch caused distraction of the attention with some subjects.

Daily averages.

The figures given in Table XIII. show that all the subjects constantly decreased the interval between beats as practice was continued. Though each practice lasted during the time required to make from 250 to 400 beats, only sections of each practice are given in the table. Section I. gives the averages of the first 40 beats; section II. the averages of the next 40 beats; and section III. that portion included between the 160th and 201st beats.

The shortening in the average time of the beat is apparent in every case. The shortening was probably due to the increasing ease with which the subject performed the experiment. His mental processes probably became more fluent as the result of practice. It was also noticed in several cases that the amplitude of the movements of the hand also de-

TABLE XIII.

Average daily rhythmical movement.

Name.	Date 1898.	1st 40 beats.	p. e.	p. e. as %	P.E.	2d 40 beats.	p. e.	p. e. as %	P.E.	beats from 160 to 200.	p. e.	p. e. as %	P.E.
	XI — 20	73	3.2	4.4	0.5	64	3.4	5.3	0.5	53	1.7	3.2	0.3
	21	53	2.4	4.5	0.4	52	2.0	3.9	0.3	49	2.1	4.3	0.3
	22	46	1.5	3.3	0.2	45	1.2	2.7	0.2	45	1.0	2.3	0.2
A. F.	23	43	1.9	4.4	0.3	42	1.9	4.5	0.3	45	2.0	4.4	0.3
	24	40	1.7	4.2	0.3	37	1.2	3.2	0.2	33	1.0	3.0	0.2
	25	36	1.2	3.3	0.2	35	1.3	3.7	0.2	34	2.2	6.5	0.4
	26	32	1.1	3.4	0.2	32	1.3	4.0	0.2	32	1.1	3.4	0.2
	27	25	1.2	4.8	0.2	25	1.1	4.4	0.2	25	1.1	4.4	0.2
	X — 25	99	3.1	3.2	0.6	91	3.0	3.3	0.5	89	3.4	3.8	0.6
	27	73	3.2	4.4	0.5	76	2.9	3.8	0.5	71	4.1	5.8	0.7
	28	69	2.6	3.8	0.4	64	2.7	4.2	0.4	61	2.2	3.6	0.4
W. S.	29	62	1.9	3.1	0.3	60	1.5	2.5	0.3	60	1.7	2.8	0.3
	30	63	2.7	4.3	0.4	60	1.8	3.0	0.3	57	1.9	3.3	0.3
	XI — 1	61	2.2	3.6	0.4	60	1.2	2.0	0.2	56	1.7	3.0	0.3
	2	59	1.5	2.5	0.3	58	1.0	1.7	0.2	55	1.6	2.9	0.3
	X — 21	84	3.1	3.7	0.5	86	2.9	3.4	0.5	87	3.3	3.8	0.5
	22	56	1.4	2.5	0.2	55	1.1	2.0	0.2	54	1.6	3.0	0.3
	23	63	1.7	2.7	0.3	65	1.7	2.6	0.3	62	2.0	3.2	0.3
C. S.	25	59	1.7	2.9	0.3	60	1.4	2.3	0.2	58	1.5	2.6	0.3
	26	55	2.4	4.4	0.4	54	2.1	3.9	0.3	54	1.7	3.2	0.3
	27	54	2.1	3.9	0.3	53	1.7	3.2	0.3	51	1.6	3.1	0.3
	28	52	1.7	3.3	0.3	52	1.2	2.3	0.2	51	1.4	2.8	0.2
	X — 23	185	4.6	2.5	0.7	175	5.2	3.0	0.9	163	5.1	3.1	0.8
	24	110	3.5	3.2	0.6	108	4.0	3.7	0.7	101	6.2	6.1	0.9
E. F.	25	133	5.5	4.2	0.9	137	3.5	2.5	0.6	143	3.7	2.6	0.6
	26	122	3.5	2.9	0.6	117	2.4	2.1	0.4	123	2.9	2.4	0.5
	27	114	2.5	2.2	0.4	112	2.0	1.8	0.3	108	2.5	2.3	0.4
	X — 23	78	3.1	4.0	0.5	75	1.9	2.5	0.3	75	3.5	4.7	0.6
	24	66	2.5	3.9	0.4	64	1.9	3.0	0.3	63	1.6	2.5	0.3
E. W.	25	70	2.3	3.3	0.4	68	1.7	2.5	0.3	63	2.0	3.2	0.3
	26	64	1.8	2.8	0.3	64	1.5	2.4	0.3	60	2.0	3.3	0.3
	27	63	1.6	2.6	0.3	62	1.2	1.9	0.2	59	1.6	2.7	0.3
	XI — 23	44	3.0	6.8	0.5	40	1.8	4.5	0.3	37	1.5	4.1	0.2
	24	40	2.0	5.0	0.3	37	1.0	2.7	0.2	33	1.0	3.0	0.2
J. P.	25	40	1.5	3.8	0.3	38	1.2	3.2	0.2	34	1.0	2.9	0.2
	26	37	1.4	3.8	0.2	36	1.0	2.8	0.2	33	1.3	4.0	0.2
	27	37	0.9	2.4	0.2	35	0.8	2.3	0.1	33	1.0	3.0	0.2
	X — 31	53	3.0	5.7	0.5	49	3.1	6.3	0.5	46	1.9	4.1	0.3
	XI — 1	56	3.8	6.8	0.6	54	2.5	4.6	0.4	50	2.8	5.6	0.5
	2	54	2.3	4.3	0.4	52	2.0	3.9	0.3	46	1.9	4.0	0.3
W. J.	3	50	2.4	4.8	0.4	49	1.8	3.7	0.3	46	1.9	4.1	0.3
	4	48	1.6	3.4	0.3	48	1.2	2.5	0.2	46	1.5	3.3	0.3
	5	47	1.2	2.6	0.2	47	1.0	2.1	0.2	45	1.6	3.6	0.3
	6	47	1.0	2.1	0.2	46	1.0	2.1	0.2	45	1.2	2.7	0.2

The unit of measurement is $1^{\Sigma} = 0.01^{s}$.

The number of experiments at each practice ranged from 250 to 400.

The probable error of a determination varied from $\pm 9^{\sigma}$ to $\pm 2^{\sigma}$.

p. e., probable error of each beat from the average on that day.

P. E., probable error of determination.

creased; this was probably due to the increasing ease of the muscular adjustments.

The results also show for most subjects a decrease in the relative probable error, but for others a nearly constant probable error. This indicates for the former class an increase in regularity. Those who did not improve to any extent in regularity were those who most shortened the average beating-time. It may be suggested that the former subjects directed their attention mainly to regularity and the latter mainly toward ease of movement. According to the record all the subjects were reported to have kept the same speed according to the directions given. Since their attention was fixed more closely on the regularity of the movement any shortening of the interval if it did not materially effect the regularity of the movement would not be noticed.

The interval for each individual, then, varies with the development of automatic control. Unless the subject is directed by some external sound to a certain time interval, until a definite muscular movement has become habitual, the tendency will be to decrease the interval between successive beats until that stage is reached when the movement is guided by the natural rate of the response of the nervous system.

In every instance, at the beginning of the series of experiments, each of the subjects had in mind some musical composition which guided him in beating time. As soon as the subject felt that he could beat time more regularly by being guided by the muscular feeling then the other was given up. When the subject thought of some musical composition the movement was much slower and more irregular than it was when it became automatic.

Preliminary tests with a sounding key showed that when the interval was guided by the ear there was a tendency to emphasize certain beats depending upon their frequency. This, of course, was due to the fact that one is accustomed to emphasize certain sounds in music and speech.

That the physiological rhythm is different from the emphasis rhythm is very clearly established by the results of this set of experiments. When the subject thought of a musical composition the movement corresponded to that which BOLTON[1] defined as the "2-groupings." However, in the latter part of the series, when the movement came to be controlled by the muscular feeling, the rate of movement corresponded to that which he called the "grouping by fours."

Our observations justify us in stating that rhythmical movement rests not only on the rate of breathing, heart-beat, etc., but also on other physiological and mental processes. It is a mental process in that the

[1] BOLTON, *Rhythm*, Am. Jour. Psych., 1893 VI 215.

duration between successive beats is mentally estimated; it is a physiological process in that the time interval can be most accurately measured by muscular action.

Relative decrease of error.

In order to see how the error was influenced by practice, the decrease of the error from day to day was computed according to the formula given on page 61. The results are given in Table XIV. The numerals

TABLE XIV.

Daily decrease of error shown in percentages.

Subject.	Section of beats.	Serial number of experiment. 1	2	3	4	5	6	7	Ave
A.F.	I	50	37	5	32	31	0	—9	21
	II	58	33	—12	35	—3	8	15	19
W.S.	I	24	32	20	—12	32	32		21
	II	38	44	49	11	39	17		33
E.F.	I	18	—10	45	29				20
	II	43	34	37	16				32
C.S.	I	34	—38	—34	—33	18	15		—14
	II	46	—47	6	—19	31	30		14
E.W.	I	34	24	26	11				26
	II	17	23	21	20				20
J.P.	I	51	37	23	34				36
	II	44	0	17	20				20
W.J.	I	32	55	33	47	31	17		36
	II	50	44	37	41	17	0		31
	Avg.	38	19	19	17	24	15		

I and II represent the percentages for the first and second groups of 40 beats respectively. The percentages in this table are based on the results given in Table XIII.

I and II, indicate respectively the first and second groups of 40 beats. Briefly, the percentages in that table show the average amounts of gain from the first, second, etc., days until the close of the series of experiments. The averages in the vertical column show that only with one subject, C.S, did practice produce negative results; and this occurred only in the first 40 beats. In the second group of 40 beats, however, the average daily gain in regularity amounted to 14%. Undoubtedly the large variations in the first group were occasioned by the irregularities in the nervous condition of the subject; it was necessary to take him at times when he was very busily engaged at work.

Two general characteristics mark the results given in Table XIV.

First. As rhythmical movement became more automatic, the percentage of decrease in the probable error proportionately decreased. This may be seen, not only by an inspection of the records for each of the

subjects, but is expressed, in a general way, by the average daily percentage of decrease as given at the bottom of the table. The same thing is seen here as in all our previous experiments and that is that the beneficial effect of practice in the reduction of the irregularities of movement is considerably greater in the first experiment than in any succeeding experiments. This decreases with considerable regularity as practice continues from day to day.

Second. The average daily gain in regularity of movement varies with each individual. The averages given in the vertical columns show variations ranging from — 14% to + 36%. The greater uniformity, as well as the usually larger percentages of the second group of 40 beats show that it requires some time before the muscles can adjust themselves to the steadiest and evenest action. Consequently the period of the first 40 beats may be considered as the training period of the muscles.

Finally, our results show that the principal effects of practice consisted mainly in overcoming the previous effects of emphasis rhythm, the rhythm of speech, poetry and music. As this was overcome the beating-time became more rapid and at the same time more regular.

VII. General summary.

The object of this investigation was to ascertain the results of practice on voluntary movements by repeating the same movements an equal number of times each day until approximately the highest degree of perfection attainable was reached.

1. *Triangular movement of the arm.*

The first experiment consisted in tapping continuously at the corners of an equilateral triangle whose sides measured 20^{cm}. The tests each day lasted only a short time; they were performed from 6 to 11 days by seven persons.

The results of the experiment showed that the greatest gains in rapidity of triangular movements of the hand as well as in the regularity of successive movements were made in the early part of the practice. The percentage of gain in speed rapidly decreased, being 20% for the second day, 10% for the fifth, and 5% for the ninth day. The probable error was used as a measure of irregularity. The percentage of decrease in irregularity of successive movements was not so large in the first part of practice as the percentage of gain in speed: but after the fourth day the percentage of decrease had grown until it exceeded the percentage of increase in rapidity, thus demonstrating that the psychological order of development in voluntary movement is (1) rapidity, and (2) regularity.

The results also showed that during each practice period the subject constantly increased in speed and regularity of movement until the setting in of fatigue. However, where the exercise was continued, after a short interval there was a renewal of the effort and the same results were observed to occur, though the period was much shorter than in the former case. These periods of renewal of energy were observed to become shorter each time until they came to effect almost every alternate movement.

II. *Drawing circles.*

This experiment consisted in making circles with the free arm movement. A true circle, drawn with a compass, 60^{mm} in diameter was placed before the subject as a copy. Preliminary tests showed that ten circles at one sitting gave the best general results. The tests were made on seven subjects, extending over six days.

The results showed that with the right hand most of the subjects gained in smoothness of contour in their drawings both during the progress of each practice and from day to day; with the left hand the results were more irregular.

Though all gained in the smoothness of contour of their curves, yet all did not make them of a size corresponding to that of the copy. These results brought out three types of practice: (1) that in which the subject decreased the size of the circle both during the progress of each experiment and from day to day; (2) that in which the size of the circle was increased during the experiment but decreased from day to day: (3) that in which there was but little variation from the copy either during the progress of the experiment or from day to day. The first two classes were those who regarded more carefully the smoothness of contour of their own drawn curves than they did their correspondence in size to that of the copy. The third class were those who directed their attention more especially to the size of the curve, and who closely observed the copy each time before beginning to draw their own curves.

The results also showed an important principle bearing on pedagogy: that a short exercise often repeated is the best method of practice for rapid development of accurate adjustment of the muscles. Long practice at writing, drawing, etc., seems to be time and energy wasted. Not only are inattentive habits cultivated, but every wrong adjustment gains a place in the chain of subconscious memories, and therefore delays the development of the control over the muscles for accurate adjustments.

III. *Development of control over untrained muscles and less adaptable joints.*

This experiment consisted in tapping continuously with the large toe

until it was completely fatigued. The make and break contacts of an electric key were connected with markers so that each movement of the key was recorded on the smoked surface of a revolving drum. In this way each phase of the toe's movement could be measured; the phases were four, namely, the downward movement, the downward rest, the upward movement and the upward rest.

The average tap-time of the subject studied was on the first day 436^{σ}; this very regularly decreased until at the close of the practice it was 212^{σ}. Likewise, the probable error decreased from 103^{σ} to 35^{σ}. Moreover, the upward rest was longer in the first part of the practice than the other three phases combined; but at the close of the series, it was the same as the downward rest, thus showing that the greatest gains in voluntary activity are those resulting from the practice of the weakest and less exercised muscles.

IV. *Estimation of time.*

After a number of preliminary tests, the intervals, 82^{Σ}, 100^{Σ}, and 164^{Σ} were chosen. The practice lasted from 8 to 16 days on seven subjects.

The results justify the following conclusions: (1) The estimate of a given interval varies for different individuals both with and without practice. (2) Practice on the same interval may cause the variations from the given interval to increase with one person and decrease with another. (3) Time estimate is a personal factor depending upon (a) the nature of the person, whether of an impulsive or quiet temperament, and (b) upon the point of the fixation of the attention, whether to the sensory or the motor side. (4) There is no "indifference point" from which the subject does not vary with long continued practice. The changes that practice produces in the estimation of time are probably due to fixing the attention on the movement to be performed, in which case the estimate is shortened in accordance with the growth of automatic control, or to the sensory side in which case the time-estimate is made longer by practice.

V. *Regulated rhythmical action.*

In arranging apparatus for this experiment the probable error was found for the Edison phonograph to range from 0.2% to 0.7%; for the Ludwig kymograph by Baltzar, from 0.2% to 2%; for a drum run by an Edison motor driven by carefully tended Edison-Lalande batteries, for 0.1% to 3%. The Pfeil marker was found at a break of the circuit to have a latent time ranging from $1.1^{\sigma} \pm 0.09^{\sigma}$ with the magnet cores distant from the armature to $14.7^{\sigma} \pm 0.03^{\sigma}$ with the cores close to

the armature. At a make the latent time ranged from $1.8^\sigma \pm 0.1^\sigma$ to $1.3^\sigma \pm 0.5^\sigma$. With this marker the make is nearly as good as the break except for its slightly greater irregularity. The DEPREZ marker from VERDIN showed a latent time at the break of $3.8^\sigma \pm 0.07^\sigma$ and of $2.5^\sigma \pm 0.64^\sigma$ at the make. Changes in the adjusting spring did not make any great changes in the figures. The probable error of the spark records was found to be $\pm 0.25^{mm}$ independent of the speed of the drum.

In beating time in unison with a sounder click each subject had his own constant error; this was generally negative, that is, the subjects generally beat time before the click occurred. With practice the constant error tended steadily to decrease, to become positive and to increase positively. The irregularity steadily decreased.

VI. *Free rhythmical action.*

The seven subjects were required to beat time without any objective signal. The interval chosen at the start was unintentionally shortened with the progress of the experiment; it was also shortened from day to day. The irregularity decreased in like manner.

NOTES.

The regular courses of the laboratory for the year 1898–99 were as follows :

1. *Physiological and experimental psychology.* Two lectures per week throughout the year. Text-books : LADD'S Outlines of Physiological Psychology, SCRIPTURE'S New Psychology. 65 seniors and juniors, 10 graduates.

2. *[illegible].* One exercise a week throughout the year. All students work simultaneously at the same exercise, each step being supervised by the in[illegible] before the next is taken. The course is designed to afford a training similar to that of an elementary course in chemistry or physiology. For the section on sight SANFORD'S Laboratory Course is used as a text-book. 14 seniors and juniors, 7 graduates.

3. *Intermediate laboratory course.* A series of 28 weekly exercises in psychological measurements. The students work in pairs at the exercises in rotation. Each exercise occupies two or three hours. The students learn the methods of measurement and computation and the use of various instruments such as the chronoscope, recording drum, etc. Text-book : SCRIPTURE'S *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 89–139. 7 seniors and juniors, 3 graduates.

4. *Advanced laboratory course.* Lectures and advanced exercises in the application of elementary mathematics in psychological problems. Text-books : FISHER'S Infinitesmal Calculus, HOLMAN'S Precision of Measurements, WEINSTEIN'S Physikalische Maassbestimungen (part of Vol. I). 4 graduates.

5. *Technical course.* This consists of a series of exercises for those who expect to teach experimental psychology and to manage a laboratory. The instruction covers : the principles involved in making, repairing and caring for apparatus, with practical training in wood and metal work ; the methods of experimental demonstration, with practice in the preparation of lantern slides and the use of lime-light and electric lanterns ; the principles of laboratory economy, etc. The workshop practice is cared for by a special instructor. The student is expected to make several pieces of apparatus involving the use of the screw-cutting lathe and the various small tools. He is urged to become sufficiently familiar with apparatus and lantern work to successfully give an illustrated lecture ; practice lectures are held and subjected to criticism. The director gives special attention to fitting the men in this course for college positions. 5 graduates.

6. *Research-work in psychology.* Participants in this course are either investigators or assistants. For assistants the object is such a training in accurate introspection, observation, experimenting and the art of research as is desirable for the general psychologist. This work is open to all. Only those who have had sufficient experience are permitted to undertake independent investigations. The result of all investigations belong to the archives of the laboratory. Those who undertake investigations thereby agree to prepare the results for publication, subject to approval, in the *Studies from the Yale Psychological Laboratory.* 6 graduates (independent investigator[illegible].

7. *Applied psychology.* One hour per week throughout the year. Application of modern psychological principles to educational subjects ; outlines of the psychology of touch, its use in education ; motor abilities, accuracy of movement, fundamental principles of writing and drawing ; sight, color-teaching ; space, form-teaching, drawing,

modeling; attention, concentration and distraction, laws for developing attention; memory, analysis into its components, experimental study of, develment and training, systems of mnemonics, time of study; imagination, use, necessity of development and repression; emotions, will; action, reflex, automatic, instinctive, voluntary, their training; education of the blind, the deaf and other defectives; principles of anthropometry and psychometry applied to study of scholars; psychological development, beginnings of instruction; economy in education, greatest results from least efforts, correlation and concentration of instruction. The course is illustrated with experiments, lantern views, and a large collection of educational material from Europe and America. 24 seniors and juniors, 9 graduates.

Some experiments by WEDENSKY, *Contribution à l' étude de l' innervation centrale*, III. internationaler Congress für Psychology, appear to have a bearing on the explanation of the work on cross-education that is being carried on at Yale. Professor WEDENSKY experimented on the cortical motor centers for the anterior limbs of the dog and the cat with the result that the state of excitation of one center played an important rôle in the modification produced by stimulation of the symmetrical center.

The date of FECHNER'S paper should be given on page 6 as 1858 instead of 1758.

STUDIES

FROM THE

Yale Psychological Laboratory

EDITED BY

EDWARD W. SCRIPTURE, PH.D.

Director of the Psychological Laboratory

1899

VOL. VII.

YALE UNIVERSITY

NEW HAVEN, CONN.

Press of
The New Era Printing Company
Lancaster, Pa.

CONTENTS

RESEARCHES IN EXPERIMENTAL PHONETICS

(*First Series*)

BY

E. W. SCRIPTURE.

The science of speech is at the present moment passing into the phase of experiment. For many years experiments have been made on the vowel sounds and on similar topics from a physical point of view, but it is only recently that the attempt has been made to arrange systematic work exclusively for the purposes of a science of speech itself.

The present study, begun in October, 1897, gives the account of some of the results already obtained (to the end of 1899) in the system of researches now in progress in the Psychological Laboratory of Yale University. The scope of these researches is far wider than the topics considered in this first report. "Experimental phonetics" would include the material of the present study but such a term would need to be extended beyond its present significance to include all the work now in progress here. I believe, however, that there will be no objection to using the name "experimental phonetics" for a science of speech in all its forms as a matter of expression. This would include not only speech sounds as material for language, but also their changes resulting from different mental conditions such as fatigue, emotion and the like; it would also include the study of rhythm in speech with its application in poetry and music.

The present investigation owes its immediate origin to suggestions from and discussions with Prof. T. D. GOODELL (Greek) and Prof. HANNS OERTEL (Comparative Philology). The question was raised concerning the possibility of using laboratory methods to settle the controversy in regard to the quantitative character of English verse. It was finally decided to study some records of English poetry made for one of the talking machines. After various trials it was found possible to obtain speech records in such a way that they could be measured.

It quickly became apparent that work on this problem required preliminary work on the elementary sounds of language. This work led to so many novelties and showed so clearly the need of revising many of our concepts of the nature of speech that the original problem was postponed

until the most valuable facts in regard to spoken sounds could be collected. These facts lay before me immediately in the records; it was only necessary to measure the sound curves and interpret them. This measuring was a most laborious and fatiguing process but after a month or two of practice in interpreting the curves the work proved to be incredibly profitable; it was rare to spend an hour at work on them without discovering some new fact. The field is, indeed, so rich and so unexplored that there is unlimited gain for any one wishing to enter it. To any one wishing to use the same methods every possible facility will be afforded by the Yale laboratory.

I. Apparatus for studying speech records.

The choice of a method for obtaining measurable records seemed to lie between:

1. Causing the sound to trace a record that might be directly studied, without the possibility of reproducing the sound.

2. Causing the sound to trace a record which could be used to reproduce the sound and which could also be studied.

Both of these principles involved most serious difficulties; a long series of investigators and inventors had, however, rendered them possible.

The *former principle* appears to have been first applied by Scott in his phonautograph.[1]

In Scott's phonautograph a large parabolic receiving trumpet carries at its end a thin membrane whose movements cause a small recording lever to write upon the smoked surface of a cylindrical drum. The sounds of the voice passing down the receiver agitate the membrane and cause the lever to draw the speech curve on the drum. A vibrating fork serves to write the time line beside the speech line. Scott was a typographer and afterwards a dealer in photographs; the instrument was made by Rudolph Koenig, the well-known maker of acoustical apparatus in Paris.

The instrument as improved by Koenig was used by Donders and others.[2]

The logograph of Barlow consisted of a trumpet or mouthpiece end-

[1] Scott, Inscription automatique des sons de l'air au moyen d'une oreille artificielle, 1861.

Scott, *Phonautographe*, Annales du Conservatoire des Arts et Metiers, Oct., 1864.

Scott, *Phonautographe et fixation graphique de la voix*, Cosmos, 1839 XIV 314.

Lippich, *Studien über d. Phonautographen von Scott*, Sitzb. d. Wien. Akad., Math.-naturw. Kl., 1864 L (II. Abth.) 397.

[2] Donders, *Ueber d. Natur aer Vokale*, Arch. f. d. holländ. Beiträge z. Natur.- u. Heilk., 1858 I 157.

ing in a thin membrane of rubber. A thin lever of aluminum carrying a point dipped in color wrote the speech curves on a band of paper.[1]

A still further improved phonautograph was used by SCHNEEBELI,[2] which carried two points, one fixed to aid in comparison and the other moving with the membrane. The inscription was made on a light strip of glass covered with a light coating of smoke and drawn on a carriage rapidly in front of the recording points. The tracings were measured with the aid of micrometric screws. SCHNEEBELI gives a number of the characteristic curves of the vowels.

Various similar methods have been employed with constantly better results. The ear drum has been used for the membrane by C. BLAKE.[3]

The hindrance due to the inertia of material levers was avoided by E. W. BLAKE, who attached a mirror to a telephone plate in such a way that a beam of light was deflected by each movement. A ray of light from a heliostat was reflected through lenses upon a photographic plate moving with a constant velocity. The sound wave thus recorded a line on the plate.[4]

PREECE AND STROH used a thin membrane of rubber stretched by a cone of paper. The cone was made to move a fine glass tube supplied with an aniline ink, the record being taken on a band of paper.[5]

RIGOLLOT ET CHAVANON covered the wider end of a paraboloid with a very thin membrane of collodion, to the center of which was fixed a small mirror working on an axis of fine thread. The deflections of the ray of light were recorded on a sensitive paper.[6]

DONDERS, *Zur Klangfarbe der Vokale*, Arch. f. d. holländ. Beiträge z. Natur. u. Heilk., 1861 III 446.

DONDERS, *Zur Klangfarbe der Vokale*, Ann. d. Phys. u. Chem., 1864 CXXIII 527.

DONDERS, De physiologie der spraakklanken, Utrecht 1870.

SCHWAN UND PRINGSHEIM, *Der französische Accent*, Arch. f. d. Studium d. neueren Sprachen, 1890 LXXXV 203.

[1] BARLOW, *On the pneumatic action which accompanies the articulation of sounds by the human voice, as exhibited by a recording instrument*, Proc. Roy. Soc. London, 1874 XXII 277.

BARLOW, *On the articulation of the human voice, as illustrated by the logograph*, Proc. Roy. Dublin Soc., 1880 N. S. II 153.

[2] SCHNEEBELI, *Expériences avec le phonautographe*, Arch. des Sciences phys. et nat. de Genève, 1878 (Nouvelle période) LXIV.

SCHNEEBELI, *Sur la theorie du timbre et particulièrement des voyelles*, Arch. des Sciences phys. et nat. de Genève, 1879 (III. période) I 149.

[3] BLAKE, *The use of the membrana tympani as a phonautograph and logograph*, Archives of Ophthal. and Otol., 1876 V No. 1.

[4] BLAKE, *A method of recording articulate vibrations by means of photography*, Amer. Jour. Sci., 1878 XVI 55; also in Nature, 1878 XVIII 338.

[5] PREECE AND STROH, *Studies in acoustics*, Proc. Roy. Soc. London, 1879 XXVIII 358.

[6] RIGOLLOT ET CHAVANON, Journal de physique, 1883 553.

The most highly developed instrument of the lever recording type seems to be that of HENSEN.[1] It consists of a membrane of goldbeater's skin in a conical form produced by molding it over a shape while moist and allowing it to dry before removal. A single light lever attached to the center of the membrane carries a fine glass thread as a recording point. It writes the curve on a thinly smoked strip of glass. The curves are studied with a microscope. This instrument has been used in several investigations.[2]

An important improvement was made in HENSEN'S recorder by PIPPING who replaced the glass thread by a small diamond which scratched the curve directly on the glass strip. With this instrument PIPPING has made a series of investigations, chiefly on the vowels.[3]

RAPPS also avoids the difficulties of a diaphragm or membrane by an ingenious optical method.[4]

The MAREY tambours in various modifications have been frequently used.[5] Other devices have been employed at different times.[6]

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[1] HENSEN, *Ueber die Schrift von Schallbewegungen*, Zt. Biol., 1887 XXIII 291; first described by GRUETZNER, Physiologie d. Stimme u. Sprache, 187, in HERMANN'S Handb. d. Physiol., I. Bd., II. Theil, Leipzig 1879.

[2] WENDELER, *Ein Versuch, d. Schallbewegung einiger Consonanten u. anderer Geräusche mit d. Hensen'schen Sprachzeichner graphisch darzustellen*, Diss. Kiel, 1886; also in Zt. f. Biol., 1887 XXIII 303.

MARTENS, *Ueber das Verhalten von Vokalen und Diphthongen in gesprochenen Worten*, Diss. Kiel, 1888; also in Zt. f. Biol., 1889 XXV 289.

[3] PIPPING, *Om Klangfärgen hos sjunga vokaler*, Diss. Helsingfors, 1890; also as *Zur Klangfarbe d. gesungenen Vokale; Untersuchung mit Hensens Sprachzeichner* (Diss. in Swedish, Helsingfors 1890), Zt. f. Biol., 1890 XXVII 1.

PIPPING, *Nachtrag zur Klangfarbe der gesungenen Vokale*, Zt. f. Biol., 1890 XXVII 433.

PIPPING, *Zur Lehre v. d. Vocalklängen*, Zt. f. Biol., 1895 XXXI 525.

PIPPING, *Phonautographische Studien über d. Quantität schwedischer Worte u. d. musikalischen Accent*, Finländska Bidrag. till Svensk Språk och Folklifsforskning, Helsingfors 1894.

PIPPING, *Ueber d. Theorie d. Vokale*, Acta Societatis Scientiarum Fennicæ, 1894 XX No. II.

[4] RAPPS, *Ueber Luftschwingungen*, Diss., Berlin 1892; also in Ann. d. Phys. u. Chem., 1893 L 193.

[5] ROUSSELOT, Les modifications phonétiques du langage, Paris 1892.

BOURDON, *L'Application de la méthode graphique à l'étude de l'intensité de la voix*, Année psychol., 1897 IV 369.

WAGNER, *Französische Quantität (unter Vorführung des Albrecht'schen Apparats)*, Phonet. Studien, 1893 VI 1.

[6] FICK, *Zur Phonographik*, Beiträge zur Physiologie LUDWIG gewidment, 23, Leipzig 1887.

KOSCHLAKOFF, *Die künstliche Reproduction u. graphische Darstellung d. Stimme*, Arch. f. d. ges. Physiol. (Pflüger), 1881 XXXIV 38.

The manometric flame method was devised by KOENIG.[1] The vowel is sung or spoken into the trumpet leading to the small box known as the manometric capsule. This box is divided in two parts by a thin rubber membrane. The part opposite the trumpet is a tight chamber through which illuminating gas is flowing: the gas is lighted at the end of the small tube. As the sound waves descend they strike the rubber membrane, set it in vibration and thus produce movements of the gas analogous to those of the air in the sound waves. By means of a revolving mirror the vibrations of the flame can be seen. These flames can be photographed[2] by selecting the right composition of the illuminating gas; cyanogen gas has been used; a mixture of hydrogen and acetylene gas burning in a chamber of oxygen seems to be successful.

The foregoing methods have been employed for the solution of the most diverse problems.[3]

The *second principle* is that of the sound-reproducing machines, or talking machines.

The original talking machine seems to have been the phonograph of EDISON. The tin-foil phonograph was afterwards superseded by the wax-cylinder form.

A sheet of thin glass receives the sound waves and engraves them in a surface of hard wax by means of a sapphire knife attached to it. By replacing the sapphire knife with a round sapphire point the glass diaphragm is made to reproduce the sound.

The great advantage of this method lies in the fact that the record can be made audible at any time; the accuracy of the result can thus be always tested.

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[1] KOENIG, *Die manometrischen Flammen*, Ann. d. Phys. u. Chem., 1872 CXLVI 161.

KOENIG, Quelques expériences d'acoustique, 46, Paris, 1882.

AUERBACH, *Untersuchungen ü. d. Natur. des Vokalklangs*, Diss. Berlin, 1876; also in Ann. d. Phys. u. Chem., 1876 Ergänzungsbd. VIII.

[2] STEIN, in MAREY, La methode graphique, p. 647.

DOUMER, *Mesure de la hauteur des sons par les flammes manométriques*, C. r. Acad. Sci. Paris, 1886 CIV 340.

DOUMER, *Études du timbre des sons, par la méthode des flammes manométriques*, C. r. Acad. Sci. Paris, 1887 CV 222.

DOUMER, *Des voyelles dont le caractère est très aigu*, C. r. Acad. Sci. Paris, 1887 CV 1247.

MARAGE, *Études des voyelles par la photographie des flammes manometriques*, Bull. de l'Acad. de Med., 1897 XXXVIII 476.

NICHOLLS AND MERRITT, *Photography of manometric flames*, Physical Review, 1898 VII 93.

[3] AUERBACH, *Die physikalischen Grundlagen der Phonetik*, Zt. f. franz. Sprache u. Lit., 1894 XVI 117.

ROUSSELOT, Principes de Phonétique Expérimentale, Paris 1897.

The phonograph has been used to receive records which have afterwards been studied.

The methods of studying phonograph records are of two kinds. Direct enlargement and measurement by means of the microscope is the method followed by BOEKE.[1] Enlargement by means of amplifying levers, recording directly on a smoked cylinder is the method used by a series of observers.[2] Phonograph records have been studied to a considerable extent.[3]

Enlargement by means of levers recording on photographic paper by means of a beam of light is the method developed by HERMANN.[4] The Yale laboratory is equipped for this method also.

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[1] BOEKE, *Mededeeling omtrent onderzoekingen van klinkerindruskels op de wasrollen van [illegible] fonograaf*, De natuur, 1890, July.

BOEKE, *Mikroskopische Phonogrammstudien*, Arch. f. d. ges. Physiol. (Pflüger), 1891 L 297.

MEYER, *Zur Tonbewegung des Vokals im gesproch. u. im gesung. Einzelwort*, Phonet. Studien, 1897 X 1 (Neuere Sprachen, IV).

[2] MAYER, *Edison's talking machine*, Nature, 1878 XVII 469.

FICK, *Zur Phonographik*, Beiträge zur Physiologie LUDWIG gewidmet, 23, Leipzig 1887.

JENKIN AND EWING, *The phonograph and vowel theories*, Nature, 1878 XVIII 167, 340, 394.

JENKIN AND EWING, *On the harmonic analysis of certain vowel sounds*, Trans. Roy. Soc. Edinb., 1878 XXVIII 745.

KLUENDER, *Ueber d. Genauigkeit der Stimme*, Arch. f. d. ges. Physiol. (Pflüger), 1879 I 119.

LAHR, *Die Grassmann'sche Vokaltheorie im Lichte des Experiments*, Diss., Jena 1885; also in Ann. d. Phys. u. Chem., 1886 XXVII 94.

M'KENDRICK, *On the tone and curves of the phonograph*, Jour. Anat. and Physiol., 1896 XXIX 583.

M'KENDRICK, MURRAY AND WINGATE, *Committee report on the physiol. application of the phonograph and on the form of the voice curves by the instrument*, Rept. Brit. Ass. Adv. Sci., 1896 669.

WAGNER, *Ueber d. Verwendung d. Gruetzner-Marey'schen Apparats u. d. Phonographen zur phonetischen Untersuchungen*, Phonet. Studien, 1890 IV 68.

[3] MARICHELLE, La parole d'après le tracé du phonographe, Paris 1897.

GELLE, L'audition, Paris 1897.

MARAGE, *Les phonographes et l'étude des voyelles*, Anneé psychol., 1898 V 226.

[4] HERMANN, *Phonophotographische Untersuchungen*, I., Arch. f. d. ges. Physiol. (Pflüger), 1889 XLV 582.

HERMANN, *Ueber d. Verhalten d. Vokale am neuen Edison'schen Phonographen*, Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 42.

HERMANN, *Phonophotographische Untersuchungen*, II., Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 44.

HERMANN, *Phonophotographische Untersuchungen*, III., Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 347.

Another of the talking machines is the gramophone. This is a development of the recording idea contained in SCOTT's phonautograph in combination with the idea of reproducing the sound in a special manner. The inventor of the method is Mr. EMIL BERLINER, of Washington, D. C. The United States patents covering the apparatus and processes are as follows: Gramophone, No. 372,786, Nov. 8, 1887; Process of producing records of sound, No. 382,790, May 15, 1888; Gramophone, No. 534,543, Feb. 19, 1895; Sound-record and method of making same, No. 548,623, Oct. 29, 1895; Gramophone, No. 564,586, July 28, 1896. These patents can be readily found in the annual reports published by the United States Patent Office.

The researches to be now reported have been made with the aid of the gramophone; an acquaintance with the principles involved in the production of the gramophone records is necessary to the proper understanding of the results obtained.

1. Making gramophone plates.

For convenience the apparatus may be divided into two sections, the recorder and the impression disc.

The recorder with which I am acquainted is that described in the Letters Patent No. 564,586; it is shown in Fig. 1. The recorder comprises a thin glass diaphragm held in a frame, Fig. 2. This frame opens on one side into a speaking tube. It is cut away on the other side to afford connection with the recording stylus. From the center of the diaphragm a metal post rises, whose free end has an axial slot into which a piece of soft rubber tube is forced and flattened. The free end of the tube receives the metal stylus, which extends outward radially and ends in a flat, sharp, flexible point. Near the middle of the stylus a hole is bored and a pin formed at one end of a metal block passes through the hole and into the central bore of a similar block. Between each block and the stylus there is a soft rubber washer. The blocks are made to clamp the stylus by means of the pointed screws passing through the support and serving as pivots.

HERMANN, *Bemerkungen zur Vokalfrage*, Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVIII 181, 543.

HERMANN, *Phonophotographische Untersuchungen*, IV., *Untersuchungen mittels des neuen Edison'schen Phonographen*, Arch. f. d. ges. Physiol. (Pflüger), 1893 LIII 1.

HERMANN UND MATTHIAS, *Phonophotographische Mittheilungen*, V., *Die Curven d. Consonanten*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LVIII 255.

HERMANN, *Phonophotographische Untersuchungen*, VI., *Nachtrag zur Untersuchung der Vocalcurven*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LVIII 264.

HERMANN, *Weitere Untersuchungen ü. d. Wesen d. Vocale*, Arch. f. d. ges. Physiol. (Pflüger), 1895 LXI 169.

These pivots form the fulcrum of the stylus. The stylus is dampened by a piece of soft rubber inserted between it and the metal cover of the sound box.

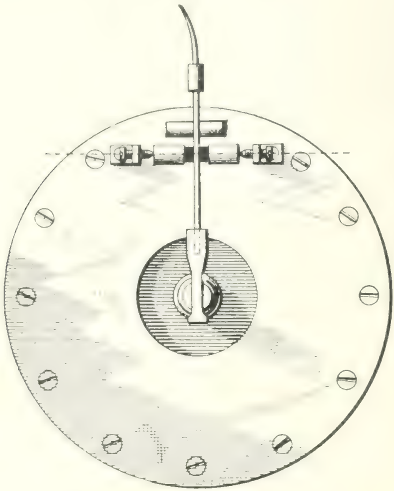


FIG. 1.

The sound waves coming down the speaking tube set the diaphragm in motion; this diaphragm moves one arm of the stylus and the point at the end of the other arm repeats this movement.

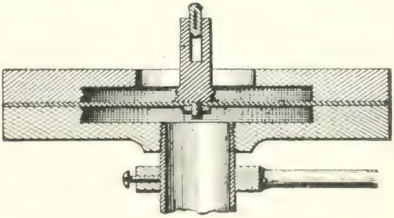


FIG. 2.

The impression disc is prepared by two methods. I shall describe first the method with which I am acquainted and then a later method which seems of special interest.

In the former method (Patent No. 382,790) a highly burnished zinc disc 18^{cm} in diameter is flowed with a saturated solution of wax in benzine; the film of wax thus deposited is so thin that the touch of a camel's hair brush marks it perceptibly.

The prepared disc is placed on a revolving plate so that its surface is touched by the point of the recording stylus (Patent No. 534,543). As the plate revolves the recorder is made to travel toward the center; thus its point cuts a spiral groove through the wax. The vibrations of the point make deflections in this groove. These deflections are in the plane of the surface of the plate and not dug into it as in the case of the phonograph.

The record disc is then placed in an etching bath similar to that used by photo-engravers (Patent No. 548,623). The part of the zinc from which the wax has been removed by the stylus is attacked by the acid and a permanent groove is made. A copper matrix is then made from this by electrolysis. The matrix contains the sound-line in relief. After the matrix has been protected by a layer of nickel, unvulcanized rubber is pressed into it. The rubber is then vulcanized in place. When removed from the matrix the rubber plate is a true copy of the original disc.

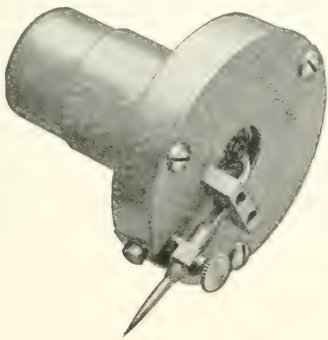


FIG. 3.

The later method of making record discs I know only from a study of the Letters Patent, No. 564,586. I judge, however, that it is a better method and I believe that it may be of easy application in the direct study of records by the microscope.

In this method a glass plate is clamped on an axis by which it can be rotated. The under-surface of the disc is carefully polished and dried

and is then covered with a thin film of linseed oil by means of a camel's hair brush. A smoky flame then held under the plate deposits a fine layer of lamp-black, thus forming an amorphous ink which covers the glass in an even, exceedingly thin layer. This coating of ink does not flow spontaneously and requires only a minute force to trace a line in it. The sound line is drawn by the point of the recording stylus in a manner similar to that just described. Copies of the disc are made by placing it over a sensitized photographic plate and proceeding by photo-engraving.

To reproduce the sound the rubber disc is placed on a plate which can be rotated by some motor power. A reproducing sound box is so arranged that the point of its stylus travels in the sound-groove. The deviations in the sound groove move the point of the stylus whereby a glass diaphragm is made to reproduce the sound waves. The reproducing sound box differs from the recording sound box chiefly in having a stiff round steel point at the end of the stylus instead of a cutting point, as shown in Fig. 3.

2. *Transcribing gramophone records.*

The speed at which the plate travels in the record-making machine is about 70 revolutions a minute. This stretches out the curves for the speech sounds so that the variations in amplitude are visible through the microscope only in the case of musical sounds and vowels. The method of direct reading by the microscope is therefore not available. The record must be transcribed in such a way that the relation between length and height, that is between time and amplitude, shall be changed. In the method about to be used the height was enlarged while the length was decreased.

In the transcribing apparatus (Fig. 4) the gramophone plate was put on a metal disc *E* similar to that of the original record-making machine. This disc was rotated at a speed of 0.1 revolution a minute by a system of spur and bevel gears. The particular system used was adopted after long experimenting; as it may be of use to others it may be profitable to briefly describe it.

A small 110-volt Edison motor *A* was connected with the electric mains through an appropriate resistance. A convenient and cheap form of resistance *L* was found in the so-called reduction sockets for 16 c. p. lamps. These contain fine resistance wire wound on asbestos, which can be placed in circuit with the lamp to any desired extent, thereby reducing the current passing through it. An appropriate plug carrying the motor wires was placed in one of these sockets; this socket was connected to another plug which was placed in another reduction socket; this finally was con-

nected to a plug placed in a socket on the main line. By moving the knobs on the reduction sockets the speed of the motor could be reduced as desired. Finally the current was passed through a 4 c. p. lamp as a permanent resistance of 800 ohms. In making the present records the motor was adjusted to about 800 revolutions a minute.

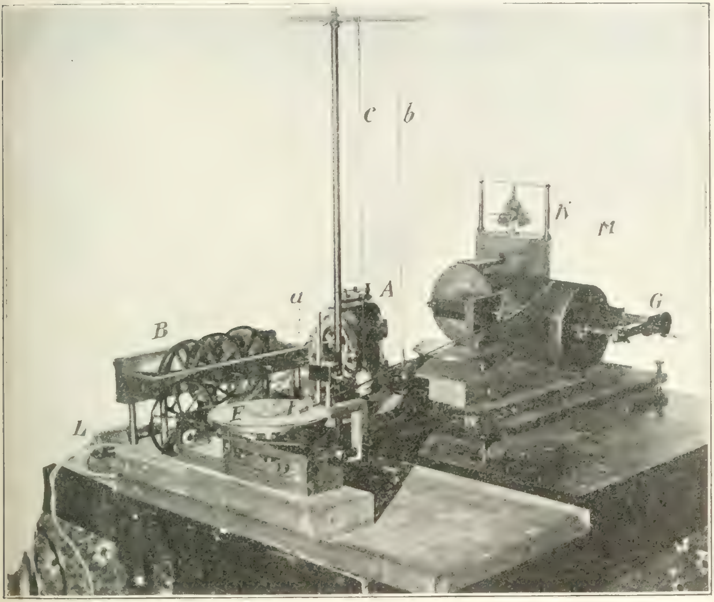


FIG. 4.

A miter gear a on the axle of the motor fitted into another miter gear on the first axle of the reducing machine B. The first axle of the reducing machine thus revolved at 800 revolutions per minute. (For still finer work it has been found convenient to use a worm on the motor axle and a worm gear on the first reducing axle; for a worm gear of n teeth the speed of the first axle is $1/n$ that of the motor.) The second axle carried a large spur gear with 160 teeth which fitted into small spur gear with 16 teeth on the first axle; thus the second axle made 80 revolutions per minute. In a similar way gear-transmission to a third axle reduced the

speed to 8 revolutions, and transmission to a fourth axle reduced it to 0.8 of a revolution. This fourth axle carried a spur gear of 20 teeth which fitted into the 100 teeth of the final driving machine of the disc whose axle thus made 0.1 revolution a minute.

The axle of the final driving mechanism carried on its further end a tube *C* with a longitudinal slit in it. Within this tube was a rod 1^{cm} in diameter with a thread of 96 turns to the inch on its surface; it was held by a nut correspondingly threaded. A projection from the rod fitted into the slit in the tube; thus the rod was forced to turn with the tube. At the same time the thread on its surface forced it to move lengthwise $\frac{1}{96}$ of an inch for each revolution. The rod bore on its end a carefully centered point and just back of this point a miter gear. The point pressed against the disc-carriage. This carriage consisted of a bar of brass running on a pair of rails and carrying the metal wheel *E*. The metal wheel rested on the carriage and its axle projected through it. As the rod traveled forward it pushed the carriage ahead of it. At the bottom of the axle there was a second miter gear *D* bearing against the first one on the rod; this turned the metal wheel in unison with the rod. When a gramophone plate was clamped on the wheel with proper centering, it was turned once in 10 minutes and was driven forward radially $\frac{1}{96}$ of an inch per revolution. Thus the speech curve on a plate would travel steadily under a fixed point from beginning to end.

Just above the disc the amplifying lever *F* was adjusted so that the soft steel point rested in the sound groove. The distance from the fulcrum to the point was 22^{mm}. The lever possessed side movement in order to transcribe the curve, and vertical movement in order to follow the changes in the thickness of the plate. The long arm of the lever reached 595^{mm} beyond the fulcrum. The extreme part of it consisted of a recording point of pendulum ribbon *M* 152^{mm} long. This point traced the side movement on the smoked paper and also yielded to the up and down fluctuations without any noticeable effect on the records. The amplification was approximately 27 times.

It was afterwards found desirable to replace the simple supporting adjustments of the steel point by an adjusting standard such as is used in ordinary laboratory work. The point could be raised or lowered by a rack and pinion and adjusted sidewise by a small screw. The vertical movement was convenient for regulating the pressure of the recording points on the drum; the rubber gramophone plates varied in thickness and would consequently raise the point more at one side than the other. This variation has been avoided in the most recently made plates.

The centering of the gramophone plate was not an easy matter. The

speech curve was made in the form of a spiral around the center of rotation in the original machine; neither the edge of the rubber disc with the record nor the hole in its center coincided with this center. To center the spiral accurately on the metal plate two methods could be used. The microscope method proved somewhat the more convenient. The metal disc was moved away from the point of the rod. A microscope or a large magnifying glass was fixed so that it was focussed on the spiral groove. As the disc was turned the groove passed through the field of vision. If the plate was not centered, it would move to one side or the other during one half a revolution; it was adjusted by the fingers until the groove did not appear to move back and forth with every turn, but to maintain a steady side movement amounting to once the width between lines for one revolution. The other method consisted in turning the disc with the recording point adjusted and noting the deviation to one side for one half a revolution. The disc was then moved radially until the point marked one half the deviation. If this was properly done, the point would show no deviation as the disc is turned.

The steel point was pressed into the groove of the plate by means of the rubber band on the thread *b*; the verticality of the pressure was assured by the plumb line *C*.

The record was made on smoked paper moved by the BALTZAR kymograph *K* in the usual way with side movement of the drum by the driving mechanism *G*.

There were such minor adjustments of recording points, levers, etc., as were requisite for accuracy and convenience. To avoid jarring through the floor the table was at a later date suspended from the ceiling by wires. The jarring of the motor was avoided by placing it on sand. The slight variations in the potential of the city current did not appreciably affect the record.

In the laborious work of transcribing these records I was greatly aided by Mr. Minosuke Yamaguchi.

The records were measured with a scale graduated in 10ths of a millimeter under a watchmaker's eye-glass or under a magnifying glass. Thus 0.1^{mm} was the unit of measurement. This represented an interval of time of $0.003\frac{1}{2}^{s}$, or $0.3\frac{1}{2}^{\sigma}$. In the case of regularly repeated vibrations the determination could be made still finer by measuring a long series of vibrations. In the calculations only the tenth of a millimeter was used. The tenths of a sigma in the results may be out by one or two units; thus a series of vibrations recorded as 2.1^{σ}, 2.1^{σ}, 1.9^{σ}, 1.9^{σ}, etc., would be possibly more correctly given as 2.1^{σ}, 2.0^{σ}, 1.9^{σ}, 1.9^{σ}, etc. These steps disappear in the plotted curves of results which were drawn smoothly by aid of rubber curves.

The calculation was aided by ZIMMERMANN's Rechentafeln and a table of reciprocals. Thus millimeter measurements were turned into periods of vibration by using the table for 35, and frequencies were found by taking the reciprocal of the period.

The reproductions of speech curves in this study were obtained by having the originals photographed, with an enlargement of four times, directly on a wooden block; the engraver then cut the line with his tool. As some of the finer details were necessarily lost in this way, the attempt was made to get larger amplification in the records. Six months of unsuccessful work with compound levers were followed by an attempt (Dec., 1899) with a single very long lever of straw having the fulcrum close to one end and the recording point of glass. This method gives most beautiful curves of the greatest delicacy; they are as large as the curves shown in the figures for *ai*, etc. below and can be reproduced directly by zinc etching. This method is being used for further researches. Many other improvements have also been lately introduced.

In addition to the illustrations produced by photography and cutting by the engraver, others have been made by drawing with the free hand on a very large scale the curve as seen through the magnifying glass; in this way the finer details could be brought out with great accuracy.

II. THE DIPHTHONG *ai* FOUND IN THE WORDS *I, eye, die, fly, thy.*

The words first studied in the present case are those of WILLIAM F. HOOLY, a trained speaker, reciting the nursery-rhyme entitled "The Sad Story of the Death and the Burial of Poor Cock Robin." The record is contained on the plate numbered 6015 made by the National Gramophone Company of New York. As it is impossible to get any definite idea of how the words actually sound except by putting the plate in the gramophone, I will try to indicate some of the characteristics of the words heard.

MR. HOOLY speaks in what appears to be the normal American accent in the neighborhood of New York except in two respects: 1. he makes an unusual effort at distinctness; 2. he recites in the manner frequently adopted by adults in speaking to children—a manner that I am able to characterize only as having an excess of expressiveness and melodiousness.

The record on the gramophone plate, as far as it has been traced off, reads as follows:

Now, children, draw your little chairs nearer so that you can see the pretty pictures and Uncle Will will read to you the sad story of the death and the burial of poor Cock Robin.

Who killed Cock Robin?
    I, said the sparrow,
    With my bow and arrow.
I killed Cock Robin.

Who saw him die?
    I, said the fly,
    With my little eye
I saw him die.

Who caught his blood?
    I, said the fish,
    With my little dish
I caught his blood.

Who'll make his shroud?
    I, said the beetle,
    With my thread and needle
I'll make his shroud.

Who'll be the parson?
    I, said the rook,
    With my little book
I'll be the parson.

Who'll dig his grave?
    I, said the owl,
    With my spade and trowel
I'll dig his grave.

Who'll carry the link?
    I, said the linnet,
    I'll fetch it in a minute.
I'll carry the link.

To extend the treatment to prose some cases of *I* were studied in another record by Mr. William F. Hooley, entitled "Gladstone's Advice on Self-Help and Thrift," being record number 6014 of the gramophone series. The speech runs as follows:

"Ladies and gentlemen, the purpose of the meeting on the 14th instant may, I can say, be summed up in a very few words: self-help and thrift."

Two examples of this diphthong were also studied in the word *thy*, as it appears in record number 668 Z (name of speaker not given), which runs as follows:

Our Father, which art in Heaven; hallowed be Thy name, Thy kingdom come . . .

In order to get some idea of the relation between the character of the vibrations and the mental character of the word I have recorded judgments

as to how the words appear to the ear. The statements are given with appended initials in the accounts of the various words : the persons observing were : (O), Hanns Oertel ; (E. M. C.), Miss E. M. Comstock; (E. W. S.), E. W. Scripture.

ai in the word *I* (first example).

The first occurrence of *ai* is in the verse *I, said the sparrow.*

A reproduction of the curve for this word is given in Fig. 5. As explained on p. 14, some of the details are lost in making the figure and others are not quite correctly given ; the original curve is much sharper and clearer.

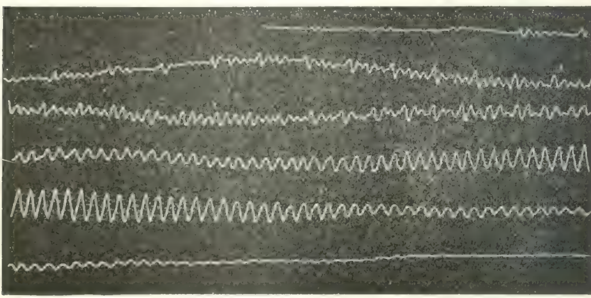


FIG. 5.

This word *I* occupies an interval of 452^σ ($^\sigma = 0.001^s$). It is preceded by a silent interval of 770^σ, or about ¾ of a second ; this is the full stop in the stanza after the question is asked and before the answer is given, indicated by ? in print. It is followed by a silent interval of 210^σ, indicated in print by a comma.

Beginning.—The beginning of the *a* is apparently clear, that is, it is not preceded by any breathing. The vocal cords are apparently adjusted for voice production before the expiration begins ; the vowel starts with a light vibration of the cords. There is no explosive sound, or glottal catch, before the vowel.

Pitch.—Beginning with a period of 18^σ, the cord tone changes slowly through 11, 10, 9, 8, 7^σ, reaching 6^σ at the 11th vibration, 5^σ at the 15th, 4^σ at the 30th ; the period of 4^σ is maintained to about the 100th vibration, after which it falls slightly to 4.2^σ during the last 7 vibrations. In other words, the pitch glides slowly upward from a tone of 56 complete vibrations per second to one of 200 per second, then more slowly to one

of 250 per second, at which pitch it remains constant except for a slight drop as the diphthong ends. Fig. 6 shows the course of the pitch-changes

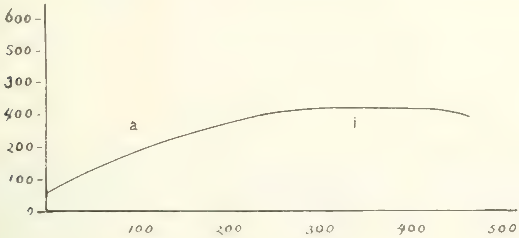


FIG. 6.

during this word. The horizontal axis in this figure, as well as in all similar ones, represents time. The point $x = 0$ is taken at the moment of the first vibration and the sound curve is supposed to be laid along the X-axis. At each point on this axis at which the curve shows a cord vibration to begin an ordinate is erected, inversely proportional to the time from this moment to the beginning of the previous vibration, that is, to the frequency of the cord vibration at that point. By an oversight the figures 300 and 400 have been interchanged.

Formation.—A drawing of the first three vibrations is given in Fig. 7; the dots indicate intervals of 1^σ.

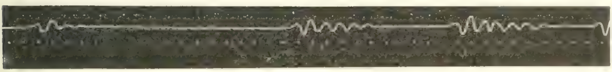


FIG. 7.

The vowel *a* begins with a movement of the vocal cords by which an extremely weak puff of air is emitted. This puff of air passing through the resonance-chamber of the mouth arouses 3 or 4 vibratory oscillations of air contained in the chamber. There is first a half oscillation of weak amplitude, then a comparatively strong oscillation, followed by very weak ones. Even the strongest is, however, very weak; the following oscillations are so weak as to be hardly perceptible. The resonance vibrations disappear and there is an interval of silence before the second puff appears. Then the cords emit another puff of air a trifle stronger than the first, the time from puff to puff being 18^σ. The 6 resonance vibrations are slightly stronger than before. The period of silence is shorter than before. The third puff occurs 11^σ after the second one. The

resonance vibrations are a trifle stronger still; there are 7 of them with a brief interval of silence. The fourth puff begins at 10^σ after the beginning of the third one. The fourth puff contains 8 resonance vibrations, all slightly stronger than before: there is no interval of silence because the fifth puff begins just as the last resonance vibration of the fourth puff ends. The interval occupied by the fourth puff is 9^σ. The end of the fourth puff, the whole of the fifth puff and the beginning of the sixth are shown greatly enlarged in the drawing, Fig. 8.

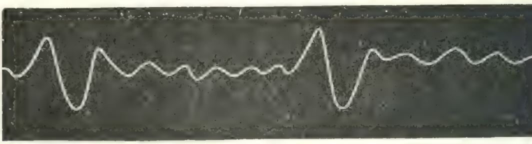


FIG. 8.

It is a characteristic trait of this particular *a* that the vibration is strongest at the start; this indicates a sudden and complete opening of the cords. The quickest opening requires, however, a little time and there must be a measureable change from no passage of air to full passage; this is shown by the weak half of the first resonance vibration preceding the large half. The form of vibration may possibly be held to indicate a complete closure of the cords whereby they actually touch each other. This is supposed to be a characteristic of spoken vowels as distinguished from sung vowels. The *a* sung by HERMANN[1] shows a gradual rise and fall of intensity such as would arise from a free vibration of the cords without touching of their edges. Spoken vowels, however, may be also produced by free vibrations of the cords as in the case of the *I* analyzed below (p. 25).

In this *I* there appears a trace of the strong secondary resonance vibration discussed below (p. 23); the phenomenon is here so faint that a discussion of it is best postponed to the study of the 2d example of *I*. The resonance tone indicated by it has a period of $3\frac{1}{2}^\sigma$, or a frequency of 286; this is approximately the note shown in Fig. 9.

FIG. 9.

The resonance vibration in the first part of the word has a period of 1^σ or a frequency of 1000. Its pitch is approximately as indicated in Fig. 10.

FIG. 10.

[1] HERMANN, *Phonophotographische Untersuchungen*, IV., *Untersuchungen mittels des neuen Edison'schen Phonographen*, Arch. f. d. ges. Physiol. (Pflüger), 1893 LIII Tafel II. HERMANN, *Weitere Untersuchungen über das Wesen der Vocale*, Arch. f. d. ges. Physiol. (Pflüger), 1895 LXI Tafel V.

As the period of the cord tone becomes shorter, the number of resonance vibrations to each period becomes smaller. Beyond the 30th period of the cord tone the resonance vibrations show a lengthening of period. In the 39th cord vibration the resonance tone reaches a period of 2.2^{σ} or a frequency of about 450; it thus falls more than an octave in the time of 9 cord vibrations, or, in this case, in 33^{σ}. Here the resonance tone is nearly but not quite of the same period as the octave, 2^{σ}, of the cord tone, 4^{σ}. This change is shown in the hand-drawing, Fig. 11, which be-

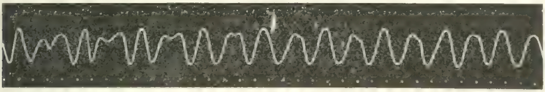


FIG. 11.

gins with the 31st vibration. This relation between resonance tone and cord tone is maintained to the end of the word; it produces the peculiar alternation of waves seen in the last two vibrations in Fig. 11.

The vibrations up to the 31st unquestionably belong to the *a*. In the vibrations beyond the 39th both the cord tone and the resonance tone are constant, except for a slight fall at the end. They unquestionably belong to the *i*. The vibrations from the 31st to the 39th show a constant cord tone and a falling resonance tone. They are presumably to be considered as belonging to the "glide." During the *a* the cords have been stretched more and more until at the 31st vibration they reach the tension required for the *i*; the only further change necessary is the lowering of the resonance tone.

Beyond the portion shown in Fig. 11 the curve shows strong vibrations so nearly alike that one is naturally induced to consider each one a cord vibration, as shown in Fig. 13. This would not be the proper way because close inspection shows that succeeding vibrations differ slightly, while alternate ones are alike. This likeness of all the resonance vibrations in the *i* as contrasted with the *a* is probably also due to a difference in the action of the cords; this difference appears more clearly in the word *eye* analyzed below, and the discussion is postponed to that point.

With the understanding that no definite limit can properly be made between one sound and the neighboring one in this case, we may, on account of the foregoing consideration, consider the *a* to have occupied the time 203^{σ} ending with the 30th vibration, the glide to have occupied 33^{σ} ending with the 38th vibration and the *i* to have occupied the remaining 216^{σ}.

The resonance tone of the *i* is one of about 450 vibrations per second, or about that in Fig. 12.

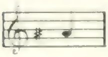


FIG. 12.

This resonance tone is much lower than the very high tone assigned to *i* by HERMANN and others but is not so low as those assigned by some other observers. There is, however, the possibility of different tones in the vowels from different speakers and also that of several resonances in the same vowel. In careful examination of the curves I find them often marked by small additional vibrations. These are frequently quite prominent

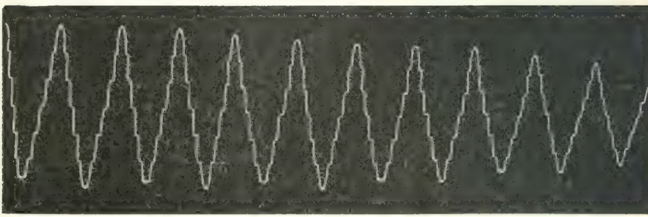


FIG. 13.

in the *i* of *ai*. Their fineness rendered it impossible to settle on any definite facts regarding them. In the drawing, Fig. 13, I have tried to give some idea of how the curve of the *i* might appear if freed from the defects of tracing. It is impossible to assign any period to these small vibrations; the regularity in the drawing was adopted for purely mechanical reasons.

FIG. 14.

.... Upper resonance tone.
– – – – Lower resonance tone.
——— Cord tone.

The changes of the cord tone and the resonance tones are indicated in a general way in Fig. 14.

Amplitude.—The amplitude of a vibration is the distance from the position of equilibrium to the extreme position on either side; it is thus one-half the difference in altitude between the crest and the trough of a wave. The course of change in amplitude is given in Fig. 15. The horizontal axis represents time as explained for Fig. 6. The vertical axis represents amplitude.

The initial resonance vibration of the first puff of this *a* has an amplitude of less than 0.1^{mm}. This slowly increases to 0.3^{mm} at the 20th vibration after which it remains practically constant to the 38th. Beyond

the 38th, that is, from the beginning of the *i*, the amplitude rapidly increases from 0.3^{mm} to 0.7^{mm} at the 50th vibration; thereafter it slowly sinks, becoming 0.3^{mm} at the 60th vibration and 0.2^{mm} at the 80th, 0.1^{mm}

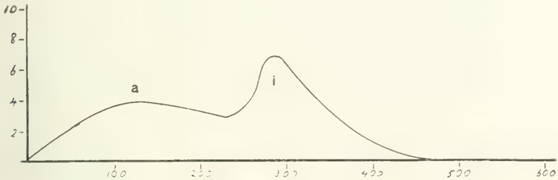


Fig. 15.

at the 88th and 0 at the 96th. The vibrations of the *i* just beyond the 50th, or the maximum of the *i*, are shown in Fig. 13; in this figure two of the large vibrations belong to one cord vibration.

The maximum for the *i* is 2⅓ times that for the *a*.

Ending.—The word *ai* ends by a gradual cessation of the expiratory impulse with hardly a noticeable change in the tension of the vocal cords; this is the clear ending usual in English. The slight fall in pitch of the *i* toward the end indicates a change that may be apparent in the auditory effect of the word, although it cannot be distinguished separately. It is probably due to a relaxation of the cords.

Relation between curve and color.—To the ear the sound of this word *I* appears from the record "colorless, without emotion, without inflectional rise or fall within the word, a monotone" (O.); "a mild statement" (E. W. S.).

The mildness of this word seems related to its length and its gradual changes in pitch and intensity.

ai in the word *I* (second example).

The second case of the word *I* occurs in the sentence *I killed Cock Robin.*

The complete reproduction of the curve is given in Fig. 16. The first five puffs are shown enlarged in the drawing, Fig. 17.

This word occupies an interval of 334^{σ}. It is preceded by a silent interval of 420^{σ}, or nearly half a second; this considerable interval would indicate a full stop. The words *With my bow and arrow* seem therefore in the thought of the speaker to belong to the previous *I*. The thought seems best indicated by a period after *arrow*; thus, *I, said the sparrow, with my bow and arrow. I killed*, etc. This second *I* is followed by

an interval of about 125^σ before any trace of the following sound can be found.

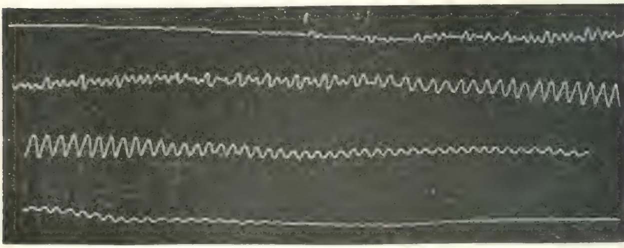


Fig. 16.

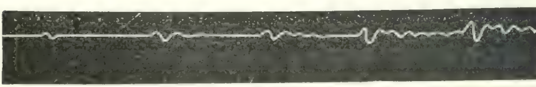


Fig. 17.

Beginning.—Similar to that of the 1st *I*, p. 16. The first five vibrations are shown in the drawing, Fig. 17.

Pitch.—Beginning with a period of 12^σ, the cord tone changes steadily through 9, 8, 8, 7, 7, 6, 6, 6, 6, 5, 5, 5, 5, 5, 5, 5, 4, 4, 4, 4, 4, 4, 4, etc., to the 48th vibration after which it slowly falls to 4.4^σ at the 70th. The course of the pitch-change is shown in Fig. 18; the plotting is done in the manner described for Fig. 6.

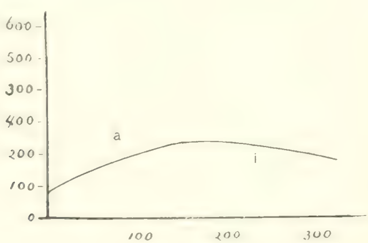


Fig. 18.

Formation.—The formation of the *a* is apparently the same as in the preceding case; the secondaries indicate a resonance tone of 1000, as in Fig. 10. At the distance of $3\frac{1}{2}^\sigma$ beyond the beginning of the vibration

there is another large oscillation markedly greater than the other secondaries, as shown in the drawing Fig. 18. This large secondary keeps at the same time behind the primary. As the pitch of the cord tone rises, the primary resonance vibrations come closer together: the large secondary, being at a constant interval behind the preceding primary, thus comes steadily closer to the following primary until it disappears in it. A drawing of two such vibrations is given in Fig. 19.

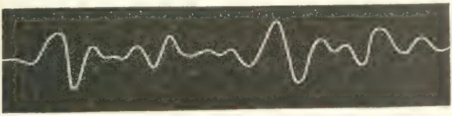


FIG. 19.

I do not believe that this larger secondary is due to an overtone-vibration of the cords. A stretched string or a reed may vibrate primarily as a whole, secondarily in halves, thirds, and so forth, producing the fundamental tone and its overtones. As the tension of the string or reed is increased, the fundamental tone rises in pitch and its overtones must do so likewise. For example, a string or a reed that vibrates in halves in addition to its fundamental vibration, will continue to vibrate in halves as the tension is changed. The curves for this vowel do not represent such a vibration. The strong secondary keeps at the same distance after the preceding primary while the distance to the following primary steadily decreases.

Two explanations of this phenomenon may be proposed.

It might be suggested that the primary and the strong secondary may represent two waves of a lower resonance while the primary and the other secondaries represent the waves of a higher resonance; this resonance would have a period of 3½ or a frequency of about 286. The note corresponding to this tone is shown in Fig. 9. It would require a rather large cavity to resonate to such a low tone. Such a cavity may perhaps arise from the pharynx and mouth acting as a single resonator of great length. There would then be at least three tones present in the *a*: the rising cord tone, the lower resonance tone of 286, which finally coincides with the cord tone, and the higher resonance tone of 1000.

Another explanation that may at least be considered is that the strong secondary arises from a flap-like action of the cords. The closure of the glottis across the air-current brings about a vibration of the edges, producing a tone whose pitch depends upon the tension of the edges. The edges can be assumed to vibrate as wholes in the manner of stretched

strings. As the tension is increased, the pitch rises. In addition to this the tissue stretching from the edges to the walls may also vibrate in unison with the edges, but just as in the case of a piece of cloth attached to a string, it may be assumed to execute an additional flap owing to the first impulse being reflected from the further walls to which the membrane is attached. If we assume that the tension of this tissue (Musculus thyreo-arytenoideus) remains constant during the vowel, this membranous flap would be independent of the tension of the cords and would follow it at a constant interval. This flap would impress itself with the air current and thus produce a stronger resonance vibration at a constant interval after the primary resonance vibration. On the assumption that the regular repetition of a sound produces a tone, the large secondary would combine with the preceding primary to produce a tone with a period of 3.5^{σ} or a frequency of about 286. Likewise it would combine with the following primary to produce a tone of changing pitch; this tone would start with a period of 5.6^{σ} or a frequency of about 178 and rise steadily in pitch till it disappeared.

The lowering of the resonance tone can be clearly seen at the 12th vibration just as at the 31st in the preceding case, although it may possibly begin earlier; it is finished at the 28th. Thus, 80^{σ} can be assigned as the time occupied by the *a*, 70^{σ} by the glide and 184^{σ} by the *i*.

Fig. 20.

The resonance tone of the *i* has a period of 1.8^{σ} or a frequency of about 555: this is approximately the note shown in Fig. 20.

The smaller vibrations are also present as mentioned on p. 20.

The changes of the three tones in this vowel are indicated in Fig. 21.

Amplitude.—The maximum amplitude in the first vibration is less than 0.1^{mm}; it increases steadily to 0.4^{mm} at the end of the *a*.

Beyond the 25th vibration the amplitude begins to increase; it reaches a maximum of 0.6^{mm} at the 31st vibration. Thereafter it decreases rather rapidly, becoming 0.2^{mm} at the 45th vibration and fading away gradually to 0 after the 75th. If the vibrations from the 12th to the 30th are to be considered as the glide, the maximum occurs just after the beginning of the *i*.

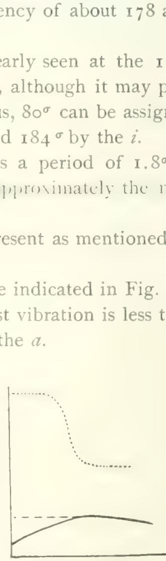


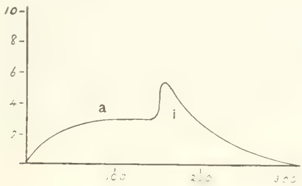
Fig. 21.

.... Upper resonance tone.
– – – – Lower resonance tone.
——— Cord tone.

The *i* is thus weaker throughout than in the previous case; its maximum amplitude is also slightly less. Owing to the loudness of the

a, the maximum amplitude of the *i* in this case is only 1½ times that of the *a*.

The course of change in amplitude is shown in Fig. 22; the plotting is done in the manner described for Fig. 15.



Ending.—Similar to that of the 1st *I*, p. 21.

Relation between curve and color.—To the ear this *I* is "shorter than the 1st *I*; more emphatic" (O.); "the word is spoken emphatically and boldly" (E. W. S.).

The emphatic character of the word may arise from its shortness, the loudness of the *a*, the quick fall of the *i*, or from other causes not determined.

ai in the word *I* (third example).

The third example of *I* occurs in the words *I, said the fly.*

This word occupies an interval of 598^σ. It is preceded by a long silent interval of 560^σ, or over ½ of a second, indicating the full stop after the question has been asked. It is followed by a silent interval of 200^σ, or $\frac{1}{5}$ of a second, indicated in print by a comma.

Beginning.—The first strong resonance vibration is preceded by 4 very small secondaries, Fig. 23. This would indicate that the expiration be-

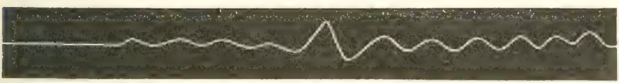


Fig. 23.

gan before the cords had closed for their first explosion but that the mouth was already in position for the vowel. Such a brief passage of air through the mouth before the cords began to vibrate would cause the resonance tone to be heard for a brief instant before the cord tone began. In this case the resonance tone began 4 thousandths of a second before the cord tone. This can hardly be considered as an extremely brief

aspirate, or *h*; the time is too short, 5^σ, for any perception of the sound distinct from the rest of the vowel.

It is quite possible that this manner of beginning a vowel may be that called by ELLIS and SWEET a "gradual glottid" and by SIEVERS a "slightly breathed beginning." "In this the cord opening passes through the positions for toneless breath and whispering before the cord tone begins, whereas the really strong impulse of expiration begins only at the moment when the voice itself sounds."[1]

Pitch.—Beginning with a period of 7.7^σ(131 vibrations per second) it rises to 7^σ at the 8th vibration to 6^σ at the 13th, to 5^σ (200 vibrations)

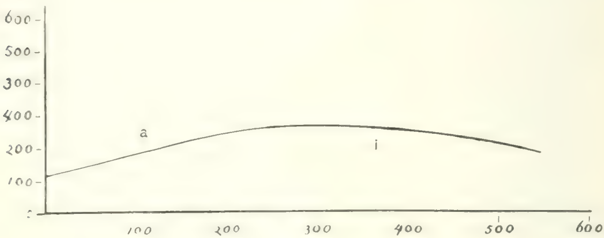


FIG. 24.

at the 20th, slowly to 3.8^σ (250 vibrations) at the 40th after which it remains constant to the 70th. Thereafter it falls slowly to 4.2^σ at the end. The course of change in pitch is shown in Fig. 24, which is plotted in the manner described for Fig. 6.

Formation.—The primary and secondary resonance vibrations are present in the *a* as in the previous cases but the secondary vibrations are relatively stronger in this case. This would indicate a more gradual opening of the cords; not so much of the energy of the puff is expended at the start, and some of it is reserved to carry the reasonance longer. There is no silent interval within the puff.

In the greater part of the curve the secondary vibrations in the *a* differ in form from those of the previous cases. They take a form that would indicate a series of partial tones differing from each other in phase by ¾ as shown in the drawing, Fig. 25.

Some of the curves for the other cases of *I* appear of the simple pendular harmonic form, but many of them show tendencies toward forms with the overtones differing in phase by ¼. Those that resemble the cases of difference by 0 and ½ cannot be distinguished from simple curves

[1] SIEVERS, *Grundzüge der Phonetik*, 4. Aufl., 140, Leipzig 1893.

on the small scale of the records. According to HERMANN the differences in phase produce no differences in the tone heard.[1] I note this particular vowel, however, as its curve differs from the others. The different forms

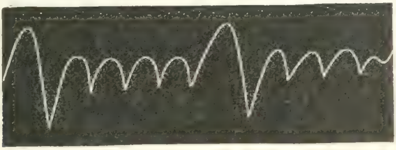


FIG. 25.

for different cases of *I* presumably indicate differences in the shape of the mouth.

The curve in this *a* presents great irregularities; they are all explainable, however, from the gradually rising pitch of the puffs whereby the number of resonance vibrations is gradually reduced as in the previous cases.

Just as in the previous cases the resonance tone begins to change while the cord tone is constant. The change begins somewhere around the 40th vibration and proceeds rather rapidly to the 50th. Thus 217^{σ} can be assigned to the *a*, 46 to the glide and 335^{σ} to the *i*.

The resonance tone for the *a* has, as before (p. 18), a period of 1 or a frequency of 1000 (Fig. 10). The resonance tone beyond the 50th vibration—which we may consider as the beginning of the *i*—has a period of 2.0^{σ}, or a frequency of 500, or approximately as indicated in Fig. 26.

FIG. 26.

The resonance tone remains constant for about 20 vibrations of the *i* and then slowly falls with the cord tone to about 2.2^{σ} at the end. The resonance tone of the *i* is very closely the octave of the cord tone.

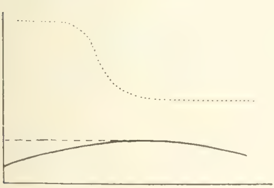


FIG. 27.

.... Upper resonance tone.
– – – – Lower resonance tone.
——— Cord tone.

The resonance vibrations of the *a* show a fairly strong secondary (p. 23) at 3.5^{σ} after the beginning. This would indicate a tone with a frequency of 286.

On the first hypothesis (p. 23) this would be the lower resonance tone, Fig. 9. On the second hypothesis it would be the constant flap tone; the changing flap tone would begin also with period of about 3.5^{σ}, and rise in pitch rapidly.

[1] HERMANN, *Beiträge zur Lehre v. d. Klangwahrnehmung*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LXV 467.

The changes in the tones of this vowel are indicated in Fig. 27.

Amplitude.—The amplitude of the maximum resonance vibration in the *a* is less than 0.1^{mm} in the first vibration; it gradually increases to 0.4^{mm} and remains constant to the end of the *a* and through the glide. After the glide the amplitude rises with moderate rapidity to 0.6^{mm} at the 62d vibration. Thereafter the amplitude falls more evenly and slowly to 0 than in the second example.

The course of change in amplitude is indicated in Fig. 28; the plotting is done in the manner described for Fig. 15.

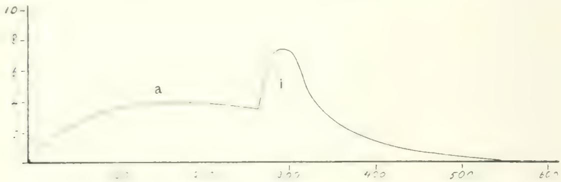


Fig. 28.

The amplitude of the *a* in this example closely resembles that in the 2d example; the *i* is also similar but its rise is more gradual and its fall more sudden. The amplitude throughout this example is a trifle less than in the first one. The maximum for the *i* is 1½ times that for the *a*.

Ending.—As on p. 21.

Relation between curve and color.—To the ear this *I* is "like the 2d but longer; a little more self-assertive" (O.); "spoken rather emphatically; like the 2d example rather than the first" (E. W. S.).

The maintenance of the pitch of the *i* may have something to do with this assertiveness.

ai in the word *I* (fourth example).

The fourth occurrence of *I* is in the line *I saw him die.* It occupies an interval of 350^σ; the word is thus shorter than any of the previous examples.

It is preceded by a silent interval of 165^σ, which is shorter than the similar interval before *I killed.* The speaker evidently feels that the words *With my little eye* belong to the following words *I saw* in making a sentence; thus no mark of punctuation should be placed after the word *eye.* This view is supported by the existence of a pause of 385^σ before the word *With.* In the previous stanza there was a pause of 770^σ after the words *With my bow and arrow* and of 0 (zero!) before them, that is, between *sparrow* and *with.* In that stanza the

speaker evidently felt the phrase beginning with *With* to belong to the preceding *I* and not to the following one. Both stanzas have been punctuated on p. 15 in accordance with these views.

The tracing of the *I* is followed by a straight line for 200^σ; this time includes the pause after the *I* and the time of the *s* of *saw*.

Beginning.—The first primary resonance vibration of the *a* is preceded by several secondaries (see Fig. 30); the beginning thus resembles that of the 3d example, p. 25

FIG. 29.

Pitch.—Beginning with a period of 9^σ it rises steadily through 8, 8, 7, 7, 7, 6, 6, 6, 6, 6, 6, 6, 5, 5, 4, 4, 4, ··· 4 (at the 28th), to 3½ at the 35th; this pitch is maintained practically unchanged to the end. In regard to pitch also this *a* closely resembles that of the 2d example but it is throughout a little higher. Starting with a frequency of about 111 it rises to about 286 and maintains this. The course of change in pitch is shown in Fig. 29, which is plotted in the manner described for Fig. 6.

Formation.—The first three vibrations are shown in the drawing Fig. 30. The motion of the cords is seen to be free and gradual as in the

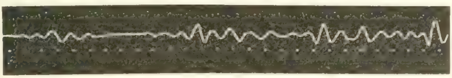


FIG. 30.

third example, p. 26 and Fig. 23. The resonance vibrations in the *a* resemble those in the 2d example in having one of the secondaries stronger than the others. This secondary maintains its place in respect to the preceding primary resonance vibration with about 3.5^σ between them. As the puffs come more rapidly, the primaries come more closely in succession, cutting off the secondaries at the end in the usual way (p. 17). Thus the larger secondary comes steadily nearer to the following primary while maintaining its constant distance from the preceding primary.

If the primary resonance vibration and the strong secondary following it indicate a tone, the period of the tone will be about 3.5^{σ} and the frequency about 286. If a tone is to be considered as being formed by the interval from the strong secondary to the following primary, it would begin at about 4.5^{σ}, or a frequency of 220, and would rise in pitch till it is extinguished. In this respect this *a* closely resembles that in the 2d example of *I* (p. 23).

It is peculiar to this *I* that the cord tone rises during the *a* to the pitch of the lower resonance tone 286 and that the *i* keeps this pitch for the cord tone.

The upper resonance tone of the *a* has at the start a period of a little over 1^{σ} or a frequency a little less than 1000. The lowering of the resonance tone may begin at the start but it cannot be detected until about the 30th vibration, owing possibly to the unusual complexity of the curve in this case. Shortly before the 40th vibration it reaches 1.5^{σ}, and at about the 48th 1.8^{σ}. Around the 50th it reaches 2.1^{σ}, at the 65th about 2.5^{σ}; after this there is scarcely any fall to the end.

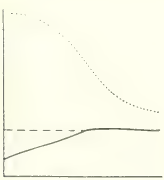


Fig. 31.

.... Upper resonance tone.
– – – Lower resonance tone.
——— Cord tone.

The changes in the tones of this vowel are indicated in Fig. 31.

Amplitude.—The maximum amplitude in the first vibration is less than 0.1^{mm}; it increases rapidly to 0.3 in the 6th vibration, reaches 0.4½ at the 17th, decreases to 0.2½ at the 28th and remains with no noticeable variation from this till the 35th. In all previous cases the *a* has steadily increased in intensity; here we have a rise and a fall.

In the *i* the amplitude rises quickly from 0.3 to 0.7 at the 42d vibration of the word (7th of the *i*) after which it sinks quickly to 0.3 at the 45th and thereafter more slowly to the end. Such a quick fall of intensity is not found in any of the preceding cases of *i*. The loud part of the *i* is shorter than in the previous cases. The maximum amplitude is reached at its 13th vibration, where it is 1½ times that of the *a*.

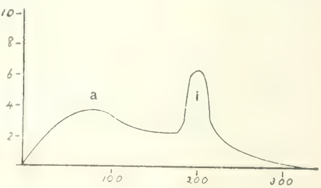


Fig. 32.

The course of the change in amplitude is given in Fig. 32, which is plotted in the manner described for Fig. 15.

Ending.—The *i* ends with a steady fall in intensity without noticeable change in pitch.

Relation between curve and color.—To the ear the word seems to be spoken "like the 3d *I*" (O.); "triumphantly" (E. W. S.). The emphatic or triumphant character of the word may be due to its shortness. The high pitch of the word and the relation of tones arising from the strong secondary may also be elements tending to make the word emphatic.

ai in the word *I* (further examples).

Nine further cases of *I* were studied, making thirteen in all. In general the fundamental characteristics of the four cases already considered were found in all the rest. Some peculiarities, however, are to be noted.

Sometimes the first vibration of the *a* is shorter than the following one. This occurs, for example, in *I'll make his shroud*, and *I'll be the parson.* In the former case the periods are 9.8^{σ}, 11.6^{σ}, 10.9^{σ}, 9.8^{σ}, etc., and in the latter 8.1^{σ}, 10.5^{σ}, 9.8^{σ}, 8.8^{σ}, 8.8^{σ}, 8.1^{σ}, etc. The cords seem to receive an excess of tension before the breath begins and to be then relaxed to the tension desired. This suggests the possibility that in all cases of *I* the tension of the cords may be made greater than desired and that it is adjusted by relaxation before the breathing begins. There are two ways of reaching an adjustment of any muscular force, one by increasing the force upward until it reaches the proper point and the other by making an excessive increase and then relaxing. This latter method is familiar in many activities. I merely suggest its possibility in speech; I see no reason for supposing it to be the method employed in the cases of *I* that do not show it in the records.

Another peculiarity lies in the ending. Most cases of *i* in *I* fade slowly away in intensity while a slight fall in pitch takes place. In the case of *I* in *I caught his blood*, the vibrations reach a maximum in the early part of the *i* as usual and thereafter decrease in amplitude; but instead of steadily decreasing to zero they are rather suddenly cut off at a point 56^{σ} beyond the maximum, at which point the amplitude is about $\frac{1}{5}$ that of the maximum. Beyond this point there are still some faint vibrations in the tracing during a time of about 10^{σ}, after which the tracing is straight. The straight tracing represents the *k*-sound in the word *caught;* the faint vibrations correspond to the glide during which the cords are still vibrating but the mouth is changing from the *i*-position to the *k*-position. The condition seems to correspond to what may be called a "sharp cut off" to the vowel (KUDELKA: "stark geschnittener Accent"[1]) in contrast with the "fading end" to the cases of *I* above.

[1] SIEVERS, *Grundzüge der Phonetik*, 4. Aufl., 204 Leipzig 1893.

In the case of *I* in *I'll make his shroud* there is also no fading away; *i* passes into *'ll* and *'ll* into *m* without any break, although a fluctuation in amplitude takes place.

In one case the fall of the upper resonance tone appears to take place from the very beginning of the word; the resonance tone is thus steadily falling while the cord tone is steadily rising. This occurs in the *a* of *I* in *I, said the fish.* The period of the resonance tone begins with 1.4^σ, reaches 1.5^σ at about the 10th vibration, 1.8^σ at the 40th vibration and then remains constant to the end of the word. The typical *a* form is lost in the curve at this point, namely the 40th vibration, or 228^σ after the beginning; the typical *i* form appears clearly after the 45th vibration, justifying us presumably in assigning 19^σ to the glide and 240^σ to the *i*.

ai in the word *I* (prose example).

This occurs in the words *may, I can say, be summed up in a very few words* of the prose speech given on p. 15.

It occupies an interval of 354^σ. It is preceded by a silent interval of not over 16^σ; the preceding sound is *ei* of *may* which fades away slowly and may occupy in extreme faintness some of this interval. It is followed by a line showing no vibrations through an interval of 70^σ; this represents undoubtedly the guttural *k* in the word *can* which seems to follow the *I* without pause as in the case mentioned on p. 31, yet the *k* does not cut off the *i* suddenly in this case as is shown by a study of the amplitude (p. 34 and Fig. 40).

Beginning.—Very faint but apparently clear, as on p. 16.

Pitch.—The successive periods are 9.8, 8.4, 7.0, 6.7, 6.0, 6.0, 6.3, 6.0, 6.0, 6.0, 6.0, 6.3, 6.7, 6.7, 6.7, 6.7, 6.3, 6.3, 6.3, 5.6 at the 20th

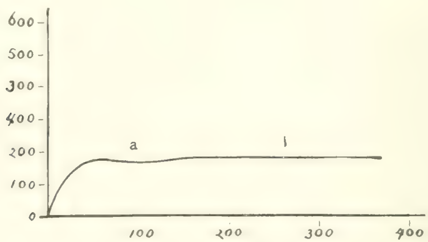


Fig. 33.

vibration; after this the period remains constant at 5.6^σ to the 68th vibration at the end of the word. This is indicated in Fig. 33, which is plotted in the manner described for Fig. 6.

Formation.—In general the curve resembles those with the strong secondary but with the difference that this secondary occurs at a smaller interval, 2.8^{σ}, after the primary. As the primary has a period of 1.0^{σ}, this produces the peculiar curve of which one vibration, is shown in the drawing, Fig. 34. This secondary is almost as strong as the primary in the early part of the *a*, but is lost sight of at a later point in the curve, possibly by coming into some relation to the upper resonance tone.

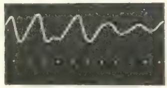


FIG. 34.

This difference from the previous cases would indicate some difference in the resonance adjustment of the mouth or in the action of the cords; it may possibly have something to do with the parenthetical character of the phrase.

The tone represented by the interval between the strong secondary and the preceding primary is constant at 2.8^{σ} or about 360, or approximately the note shown in Fig. 35. The resonance tone of the *a* starts at 1.0^{σ} or 1000, as in the first example, p. 18, being indicated approximately in Fig. 10.

FIG. 35.

At about the 17th cord vibration the resonance tone begins to fall in pitch. As its period becomes longer, it more nearly coincides with the period between the strong secondary and the preceding primary; the curve becomes smoother and loses the little notch after the primary. The 20th vibration is shown in the drawing, Fig. 36. The resonance tone continues to fall slowly but steadily to the end of the *i*, reaching 2.8^{σ} or about 360 at the end; this is, curiously enough, the pitch of the lower resonance tone of the *a* (Fig. 35). The curve at the point where the *i* has fallen greatly in amplitude and the period of the resonance vibration is somewhat less than half that of the cord vibration is shown in the drawing, Fig. 37.

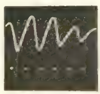


FIG. 36.

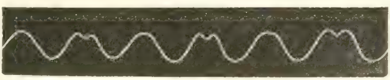


FIG. 37.

We are perhaps justified in placing the end of the *a* at the point where the resonance tone begins to fall, that is, at the 17th cord vibration; this would give *a* a length of 116^{σ}.

The vowel *i* thus continues the constant pitch of the *a* and also the

drop of the resonance tone in the glide. It is thus quite impossible to assign any limit between the glide and the *i*. Even the peculiar increase in amplitude that characterizes all the previous cases of *i* in *I* is here so gradual that it cannot be used to mark the limit (see Fig. 40).

FIG. 38.

The remarkable fall of the resonance tone from 1.0^σ, or 1000, in the *a* throughout the *i* to its end at 2.8^σ, or 360, at the end, extends over about the musical interval of a duodecime, or approximately as indicated in Fig. 38.

The changes in the three tones of the *I* are indicated in Fig. 39.

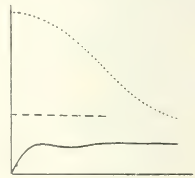


FIG. 39.

.... Upper resonance tone.
– – – – Lower resonance tone.
——— Cord tone.

Amplitude.—The amplitude increases rather steadily at first, then rapidly in the early part of the *i* and falls rather more rapidly than usual to the end. This is indicated in Fig. 40, which is plotted in the manner described for Fig. 15. The maximum amplitude for the *i* is about 1½ times that for the *a*.

Ending.—A fall of amplitude to 0 without any fall in pitch of the cord tone, as in the 4th example, p. 31.

Relation between curve and color.—To the ear this word is "colorless, unemphatic" (O.); "short, high, colorless, firm, a statement of no particular importance" (E. W. S.). It seems impossible to find any relation

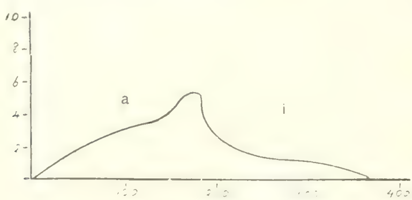


FIG. 40.

between these judgments and the recorded curve. Shortness was noted above (p. 31) as connected with emphasis; the unemphatic *I* (first example) was long and had a different curve of pitch. The very peculiar change in the resonance tone may by future collation with similar cases be found to be connected with the color of the word.

ai in the word *eye.*

The word occurs in the line *With my little eye*. A reproduction of the

curve is given in Fig. 41; the first few vibrations of the *a* are not very satisfactorily shown in the cut.

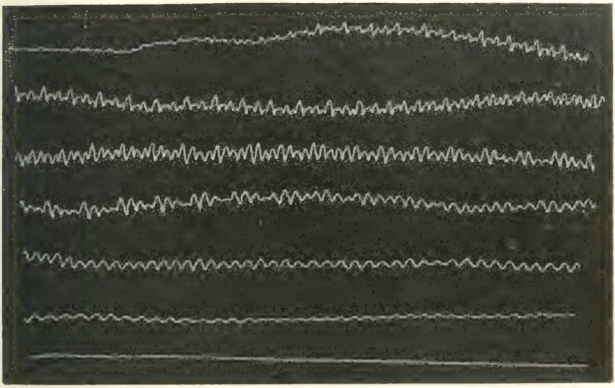


FIG. 41.

It occupies an interval 556^{σ}. It follows immediately on the last vibration of the *l* in the word *little*. The three words *my little eye* are here spoken with no separation. It is interesting, in passing, to consider the possibility that this fusion of the three words go parallel to a fusion of thought. It is evident from the very tone of the speaker that he is thinking of one thing, a certain *eye*, and that the facts of *mine* and *smallness* are not of any particular account to him.

The word *eye* is followed by a pause of 165^{σ} before the word *I* (see p. 28) which does not seem sufficient to justify a comma.

Beginning.—The faint vibrations of *l* in *little* die away just before the first primary resonance vibration of *eye* appears. The *a* begins as in *I*, 1st example, p. 16.

Pitch.—The vibrations of the preceding *l* decrease in amplitude until the line shows only a faint wavering. The first indication of *a* is a single resonance vibration on the line; this is repeated after 2.5^{σ}, and again after 3.9^{σ}. From this point the *a* curve clearly appears. It slowly falls in pitch to a period of 4.2^{σ} at the 20th vibration, 4.6^{σ} at the 40th, 4.9^{σ} at the 50th, 5.3^{σ} at the 54th, 5.6^{σ} at the 60th, 6.3^{σ} at the 66th, 6.7^{σ} at the 70th and 7.0^{σ} at the 73d. From this point onward the pitch continues to fall slowly, reaching 8.4^{σ} at the 80th vibration and ending with about 11^{σ}.

In pitch this *ai* differs radically from all the other examples; it starts with a moderately high pitch and falls continuously. The course of the change in pitch is indicated in Fig. 42, plotted in the manner described for Fig. 6.

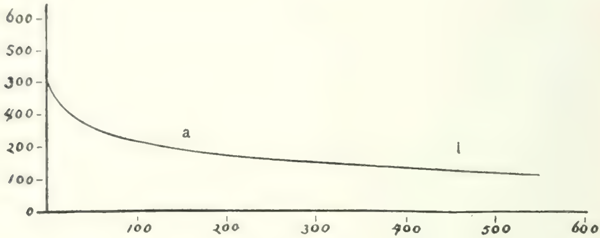


FIG. 42.

There is the possibility that the fall in pitch in this word may have something to do with its position at the end of a phrase. If the word had been followed by a long pause, it would naturally have fallen on account of its position at the end of a sentence; the pause, however, was extremely short and we cannot very well assume a short pause as the equivalent of a period unless we give up the accepted theory of relation between punctuation and time. It is, nevertheless, possible that this theory may have to be modified as later researches have shown that comma pauses may be long and semi-colon and colon pauses may be very short. I am inclined to think, however, that the true explanation is to be found by supposing the *ai* in *eye* to be a phonetically different sound from the *ai* in *I*, although the ear may not clearly distinguish between them. This point will be spoken of below in the section on general observations on *ai*.

Formation.—In the portion from the beginning to the 43d cord vibration the formation resembles that of the 2d and the prose examples of *I* in having a large secondary resonance vibration at a constant distance after the beginning of the primary one: this constant distance represents a period of 2.3^σ or a frequency of 435 (indicated in Fig. 43) as contrasted with the period of 3.5^σ (frequency of 286, Fig. 9) for the former and 2.8^σ (frequency of 360, Fig. 35) for the latter. After the 43d vibration there is a change in the curve indicating a change in this large secondary; apparently it decreases and disappears but I have not been able to decide with any confidence just what happens. After the 43d vibration the curve resembles that shown in Fig. 25.

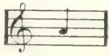


FIG. 43.

The resonance tone of the *a* has a period of about 1^σ or a frequency of about 1000 (Fig. 10). At about the 40th cord vibration the period begins to lengthen, becoming 1.8^σ at the 63d, 2.1^σ at about the 77th, after which it continues to fall slowly to 2.5^σ at the end. The resonance tone of the *i* is thus on an average about the same as the lower resonance tone of the *a* (Fig. 43).

In spite of the fact that the fall in resonance begins at about the 40th vibration, the curve maintains its typical *a* form till after the 70th vibration. Beyond this point there is a decided difference, which is fairly well apparent in Fig. 41. The primary resonance vibration is of about the same amplitude as that of the *a* but the secondaries are all nearly as large as the primary. Such a difference might possibly be explained by a difference in the action of the vocal cords. The following theory is proposed. In the *a* they vibrate so that the air current is entirely cut off at one point in each vibration; the pressure of the air forces them outward suddenly, producing a strong puff after which there is an interval before the cords again strike and cut off the air. This puff sets the air in the resonance chamber into vibrations that decrease in amplitude. As long as this complete closure occurs, any increase in the force of expiration will increase the force of the puff and of the primary and secondary resonance vibrations in approximately the same ratios. Increased force will change the amplitudes without essentially modifying the original form of the curve.

During the *i* there is no such great predominance of one resonance vibration over the others; the secondary resonance vibrations are nearly as strong as the primary. This is the case also in all the examples of *ai* studied above, but here it is very striking on account of the fact that the cord period for the *i* is longer and not shorter than that for the *a*; there can thus be no attempt at explanation of the strength of the secondaries by the assumption of force gained by the shortening of the cord period. The explanation rather seems to lie in a different action of the cords. The following theory is suggested. In the formation of this *i* the cords do not strike or entirely close the air passage and thus the emission of air at the beginning is strong and steady rather than explosive; the first resonance vibration would thus be somewhat stronger than the following ones but all would be nearly alike. The increased force in the *i* would make all of them nearly as strong as the primary of the *a* as in this word, or even far stronger than in the cases of *I* studied above.

The changes within this word are so gradual that any assignment of definite limits for the *a* and the *i* would be apparently capricious. The distinct *a* character appears to my eye to be lost somewhere after the 66th

vibration and the distinct *i* character to begin somewhere about the 72d. If these points are selected as limits—an action that is hardly justifiable—the *a* would occupy an interval of 315^σ, the glide 35^σ and the *i* 206^σ. The *a* is at any rate longer than the *i*, in quite a marked opposition to the cases analyzed above.

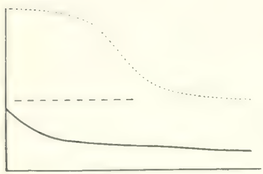


Fig. 44.

.... Upper resonance tone.
– – – – Lower resonance tone.
——— Cord tone.

The changes of the three tones in the *I* are indicated in Fig. 44.

Amplitude.—The *a* rises from zero as usual to an amplitude of 0.5^{mm} at the 17th vibration and remains practically constant to about the 66th vibration, after which there is a slow decrease to zero at the end. There is not the rapid increase to a maximum in the *i* found in the cases of *I* studied above. The maximum for the *i* is somewhat less than that for the *a*. The course of the change is indicated in Fig. 45, which is plotted in the manner described for Fig. 15.

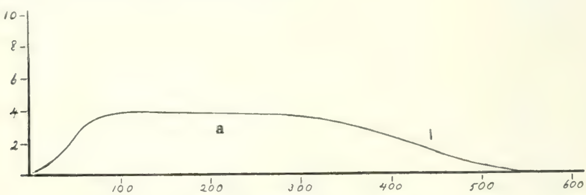


Fig. 45.

Ending.—This occurs by a fall of the amplitude to zero.

Relation between curve and color.—The ear notices that this word appears "weaker than the preceding *I*'s and also than the cases of *die*; lower in pitch" (O.): "somewhat higher in pitch than most of the *I*'s but not so high as the immediately following *I*; a somewhat colorless and unimportant word, differing quite from the modulated, flexible *fly* just preceding" (E. W. S.). The weakness of the word seems related to the falling pitch and the weakness of the *i*. The words *die* and *fly* are considered below. To the ear there is no essential difference between the *ai* in *I* and that in *eye*, yet the speaker makes a difference as indicated by the curves of results for pitch and amplitude.

ai in the word *die* (first example).

This occurs in the phrase *Who saw him die?* The word occupies an

interval of 510^σ of which 47^σ belong to *d* and 463^σ to *ai*. The curve of the entire word is reproduced in Fig. 46.

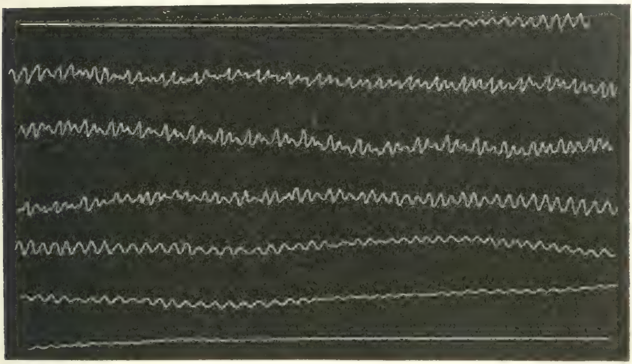


FIG. 46.

Beginning.—The word begins with 20 vibrations belonging to the *d*. These vibrations have a period of 2.0^σ or a frequency of 500. At the present moment it is impossible to say whether these are resonance vibrations imposed on a cord vibration or separate cord vibrations; it is quite probable that they are cord vibrations as they have no appearance of being grouped as is the case in resonance vibrations imposed on cord vibrations.

The amplitude increases rapidly from zero to 0.3^{mm} at the end of the *d*.

Immediately after the strongest vibration of the *d* there follows a set of strong vibrations showing the *a* form.

In speaking the word *die* a decided movement of the larynx can be felt with the fingers; this would indicate a considerable difference between the tension of the cords for *d* and that for *a*. The period of this first vibration is 3.2^σ; its amplitude is 0.3^{mm}. The *a* thus begins promptly and loudly, as might be expected from the fact that the expiration is already in progress and the cords are already in vibration. The pitch of the *a* in the first vibration is higher than in the subsequent vibrations as might be expected on the assumption that the cords are already stretched to give a period of 2.0^σ for the *d*, and must be relaxed to produce the lower tone of the *a*. While this relaxation is going on, the cords must pass through all intermediate positions between that for a period of 2.0^σ and that for one of 3.2^σ. This occurs to a large extent apparently within

the time required for the vibrations of the *d*. At the same time the mouth is changing from the *d* position to the *a* position. These facts seem sufficient to explain the curve of change in the drawing, Fig. 47;

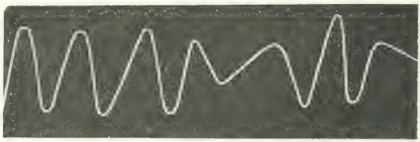


Fig. 47.

the three vibrations on the left are the last of the *d*, the strong one on the right is the primary resonance vibration of the first puff of the *a* and the connecting line shows the curve during the glide.

Pitch.—The successive periods of the cord vibrations are 2.8, 3.2, 4.9, 5.6, 5.3, 4.6, 4.4, 4.2, 4.2, 4.2, 4.2, 4.2, 4.1, 4.1, 4.0, 4.0, 4.0, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 4.1, 4.1, 4.2, 4.2, 4.2, 4.3, 4.3, 4.3, 4.4, 4.4, 4.5, 4.5, 4.6, 4.6, 4.6, 4.6, 4.6, 4.7, 4.7, 4.7, 4.7, 4.8, 4.9, 5.0, 5.1, 5.3, 5.3, 5.3, 5.3, 5.3, 5.3, 5.5, 5.7, 5.9, 6.0, 6.1, 6.3, 6.5, 6.7, 7.0, 7.2, 7.4, 7.4, 7.5, 7.6, 7.7, 8.1, 8.4, 8.8, 8.9, 9.1, 9.5, 9.8, 10.5, 10.9, 11.2, 12.3, 13.0. These figures may be 0.1^{σ} either side of the correct values as, owing to instrumental difficulties, the curves could not be read to a smaller unit than 0.1^{mm}.

The pitch thus quickly descends from the tone of 500 vibrations for the *d* to one of 179, then ascends to one of 257 and then again descends slowly to the very low one of 77. These changes are shown in Fig. 48, which is plotted like Fig. 6.

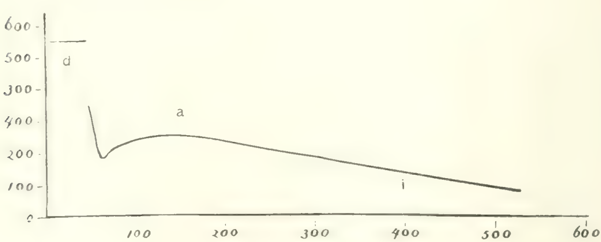


Fig. 48.

Formation.—The *a* portion of the curve resembles that of *I*, 1st example, p. 17. The resonance vibration in the first part has a period of

1^σ or a frequency of 1000, as in the first *I*, p. 18, Fig. 10. At about the 40th cord vibration it is lengthened to 1.4^σ, at the 55th to 1.6^σ, at the 58th to 1.8^σ; after this it changes slowly reaching 2.1^σ at the 75th and increasing but little more to the end at the 86th.

At about the 52d vibration the curve, while still retaining the *a* form, appears to begin to take on the *i* character as described on p. 19; the *i* character appears fairly complete at about the 57th vibration. Although no definite limits are to be made, we can assign very roughly 240^σ to the *a* and 220^σ to the *i*, or about half of the time to each.

No trace of a strong secondary resonance vibration in the *a* portion can be detected. The *a* starts at a pitch too high for the lower resonance tone found in the previous cases, but even after the pitch has fallen this tone seems to be absent.

A rather peculiar distribution of amplitude among the resonance vibrations can be seen in the *a* portion in Fig. 46. Although the puff for the cords is strong and sudden, as indicated by the large abrupt primary resonance, yet the force of the puff is not so quickly exhausted as in previous cases, as indicated by the greater size of the following resonance vibrations. The second case of *die* (below) resembles this one in this respect.

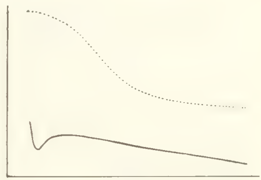


FIG. 49.

.... Resonance tone.

——— Cord tone.

The changes in pitch of the two tones of this *ai* are indicated in Fig. 49.

Amplitude.—The vibration begins with an amplitude of 0.3^{mm} for the primary resonance vibration which becomes 0.4^{mm} at about the 35th vibration; it sinks thereafter very slowly to zero at the end. The maximum amplitude is thus found in the *a* and there is no such sudden rise as is found in all the cases of *I* above. The course of change is indicated in Fig. 50 plotted like Fig. 15.

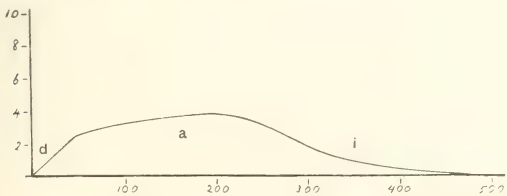


FIG. 50.

Ending.—The *i* ends with a fall in both pitch and amplitude, indicating simultaneous relaxation of the cords and the respiratory pressure.

Relation between curve and color.—The effect on the ear is that of "more emphasis at the beginning with decrease toward the end" (O. and E. W. S.). The high pitch of the *d* and the *a* at the start seem to correspond to the word-color.

ai in the word *die* (2d example).

This occurs in the phrase *I saw him die.* The entire word occupies an interval of 504^σ, of which 28^σ can be assigned to the *d* and 476^σ to the *ai.* The entire curve is reproduced in Fig. 51.

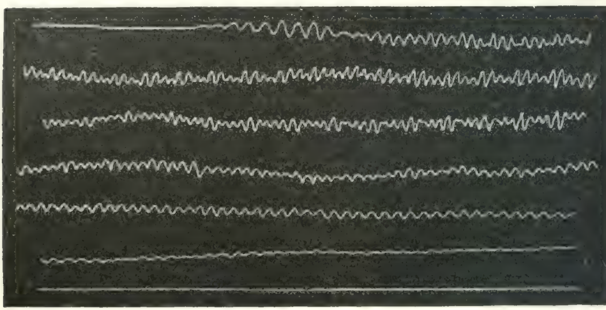


FIG. 51.

Beginning.—The word begins with 11 vibrations rapidly increasing in amplitude from 0 to 0.4^{m} and having a constant period of 2.5^σ, or frequency of 400. These are the vibrations for the *d*; they resemble those of *die*, 1st example, p. 39.

The sudden fall in pitch after the *d* is quite marked. The *d* curve is lost at once. The following interval of 7^σ can hardly be said to be the first vibration of *a* as its secondaries are very irregular in form; during this interval the mouth is changing from the *d* shape to the *a* shape. The peculiar form of the vibration is well shown in Fig. 51; the secondaries of the first few *a* vibrations are, however, slightly more prominent than in the original curve.

Pitch.—The successive vibrations of *ai* occupy periods measuring 8.4, 7.7, 4.6, 4.2, 4.2, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.9, 5.3, 5.3, 5.3, 4.9, 4.9, 5.3, 5.3, 5.3, 5.3, 5.3, 5.3, 5.3, 5.3, 5.6, 5.6, 5.6, 6.0, 6.0, 6.3, 6.7, 6.3, 6.0, 6.3, 6.3, 6.7, 7.0, 7.0, 7.0, 7.0, 7.4, 7.7, 7.7, 7.7,

8.4, 8.4, 8.4, 9.0, 9.5, 10.5, 10.5, 10.5, 11.2, 11.6, 12.3, 12.3, 12.3, 13.0, 14.0, 14.0, 14.7, 15.8, 15.8, 15.8, ?. As previously explained p. 13, these figures may be in error by one or two tenths of a sigma, or in ten-thousandths of a second. The pitch of the cord tone thus descends as low as a frequency of 63. The general course of pitch is shown in Fig. 52 plotted like Fig. 6.

FIG. 52.

Formation.—The *a* curve differs from that of most cases of *ai* in having less difference between the first resonance vibration and the rest; the first and second are, in fact, of almost equal intensity. This would indicate a more gradual opening of the cords with less explosive effect. The *a* thus does not differ so much from the *i* as in most cases. Another case of *i* like this is found in the first example of *die* (above) and in *thy* (below).

The resonance vibration in the *a* has a period of 1^σ or a frequency of 1000 at the start (Fig. 10). It falls steadily, reaching a period of 1.4^σ around the 20th vibration, 1.8^σ around the 40th, and 2.1^σ around the 60th, which is maintained to the end. There is no indication of a lower resonance tone.

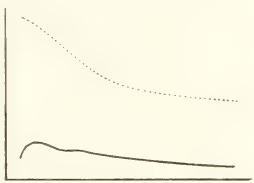


FIG. 53.
.... Resonance tone.
——— Cord tone.

The curve changes from the *a* form so gradually to the *i* form that it is quite impossible to place any dividing lines; each element of the diphthong may be said roughly to occupy half the total time.

The changes of the two tones are indicated in Fig. 53.

Amplitude.—The amplitude of the strongest resonance vibration begins at 0.3 and is maintained with fair consistency for about half the *ai*; after this it slowly falls to zero. This curve is given in Fig. 54 plotted like Fig. 15.

Ending.—The *i* ends with a fall in both pitch and amplitude, indicating a simultaneous relaxation of the cords and the respiratory pressure.

Relation between curve and color.—To the ear "it does not rise to a high pitch but starts with it and maintains it better than the other word

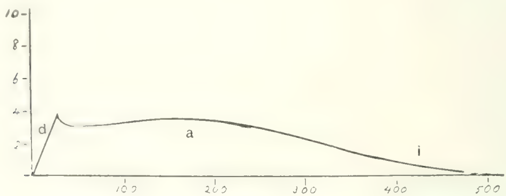


FIG. 54.

die" (O.); "it starts high and steadily falls" (E. W. S.). The apparent high start is probably due to the pitch of the *d*.

ai in the word *fly*.

This occurs in the phrase *I, said the fly*. The curve for *ly* occupies an

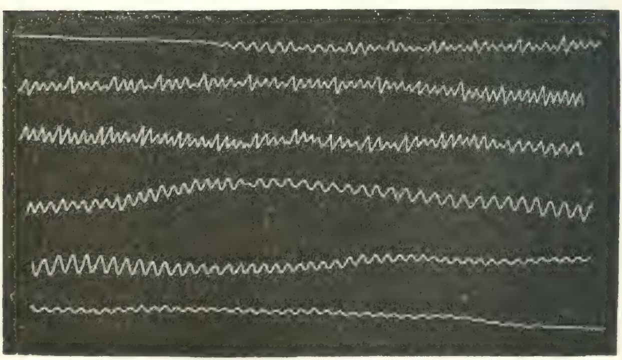


FIG. 55.

interval of 489^σ of which 25^σ belong presumably to the *l* and 464^σ to the *ai*. The curve is given in Fig. 55. It is followed by a silent interval of 371^σ which is longer than the comma pauses mentioned above (p. 16) and shorter than the full stop (pages 22, 25).

Beginning.—No specific details concerning the *f* can be derived from the curve. The strong vibrations just preceding those of the *a* are presumably from the *l* sound. They rise rapidly in intensity and greatly resemble those of the *d* in the two cases of *die* above; their period is 1.9^σ and their frequency 526.

Immediately after the last vibration of the *l* there follows a short *a* vibration with primary resonance vibrations not so strong as in the following ones. The cord adjustment seems not to be perfected for the *a* till the second characteristic *a* vibration occurs; this is well shown in Fig. 55.

The *a* begins promptly and loudly after the *l*.

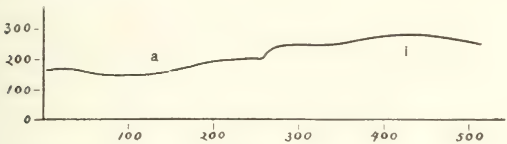


FIG. 56.

Pitch.—The successive periods of the cord vibrations are 6.0, 6.3, 6.3, 6.3, 6.3, 6.3, 6.7, 6.7, 6.7, 6.7, 6.7, 6.7, 6.7, 6.7, 6.7, 6.7, 6.3, 6.3, 6.3, 6.0, 6.0, 6.0, 6.0, 6.0, 5.8, 5.8, 5.8, 5.8, 5.6, 5.6, 5.6, 5.4, 5.3, 5.3, 5.3, 5.3, 4.9, 4.9, 4.9, 4.9, 4.9, 4.9, 4.9, 4.9, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.5, . . . (retaining this period for 27 vibrations) . . ., 3.9, 4.2, 4.2, 4.6, 4.6, 4.6, 4.6, 4.6. There is a rather sudden, though small, change in period from 4.9 to 4.2; this occurs at the irregular place a little to the left of the middle of the fourth line of the curve in Fig. 55. This is due presumably to a rather sudden tightening of the cords for the *i*. The course of change in pitch is shown in Fig. 56, which is plotted like Fig. 6.

FIG. 57.

Formation.—The *a* portion of the curve resembles that of the 2d example of *I* (p. 22), with a specially strong secondary resonance vibration at 3.9^σ after the primary, representing a tone with a frequency of 256. This is lower than in any of the previous cases (Fig. 57).

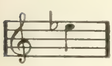


FIG. 58.

The resonance tone begins with a period of 1.6^σ or a frequency of about 625 (approximately as in Fig. 58). This falls slowly reaching 1.8^σ at about the 35th vibration, 2.0 at about the 40th, 2.2 at about the 50th, and 2.5 at the end. This indicates a resonance tone about the same as that of *i* in *eye* (p. 36, Fig. 43).

The change from *a* to *i* proceeds in general as in all the other cases but the change in curve-form seems a little more marked. It may be said to occur at the 43d vibration, or 301^{σ} after the beginning and 163^{σ} before the end.

The changes in the three tones are indicated in Fig. 59.

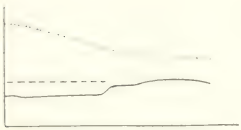


Fig. 59.

.... Upper resonance tone.

– – – – Lower resonance tone.

——— Cord tone.

Amplitude.—The *a* begins with an amplitude of 0.2^{mm} for the strong resonance vibration in the first puff from the cords, 0.3^{mm} for that in the second puff and rises quickly to $0.3\frac{1}{2}^{mm}$. After remaining fairly constant for a while, it becomes 0.3^{mm} toward the end of the *a*. In the first part of the *i* it rises to 0.4^{mm}, after which it gradually falls to zero at the end. The change of amplitude shown is in Fig. 60, which is plotted like Fig. 15.

Ending.—The *i* ends, like most of the cases examined, in a combined fall in pitch and intensity.

Relation between curve and color.—To the ear this word has a fall-and-rise of intonation like that of *well* and *yes* in such dubitative as *Well, you may do so if you wish, but I would prefer not. Yes, it may very well be true although we have no evidence for it* (O. and E. W. S.).

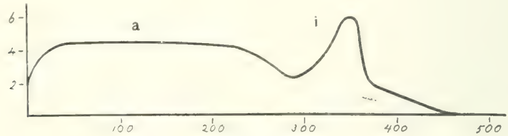


Fig. 60.

The word *fly* appears to sink and then rise in intonation to a greater degree than the corresponding words *sparrow, fish,* etc. This fall-and-rise is due to the fall from a tone of the frequency 526 of the *l* to one of 160 at the beginning of the *a* and then the rise in the *a* and *i* as shown in Fig. 56. Probably the reason for the rise in the *i* is to be found the rising intonation usual in English at the end of a parenthetical clause ;[1] that the clause *said the fly* is such a one inserted in the statement *I, with my little eye, I saw him die* seems indicated also by the fact that the silent interval after *fly* is less than that usual for a period. If *said the fly* were not parenthetical, there would probably be a longer pause

[1] Sweet, New English Grammar, § 1946, Oxford 1898.

after *fly* and it would have a falling instead of a rising intonation. In this case the lines would read: *Who saw him die? I, said the fly`. With my little eye I saw him die.* If special weight is to be given to the falling intonation of *eye* (p. 36) as opposed to the brevity of the pause after it, then *eye* would be considered as ending a phrase. The reading required by the intonations of *fly* and *eye* would thus be: *Who saw him die? I, said the fly´, with my little eye`. I saw him die.*

ai in the word *thy* (first example).

The word occurs in the phrase *Hallowed be thy name* on the gramophone record plate described on p. 15. Much of the work on this word has been done by Miss E. M. COMSTOCK. The entire curve is reproduced in Fig. 61.

The time occupied by the word is 505.8^{σ}. It is preceded by a silent interval of 73.5^{σ}. It is followed by an interval of 145.3^{σ} before any trace of the *n* of the following word appears.

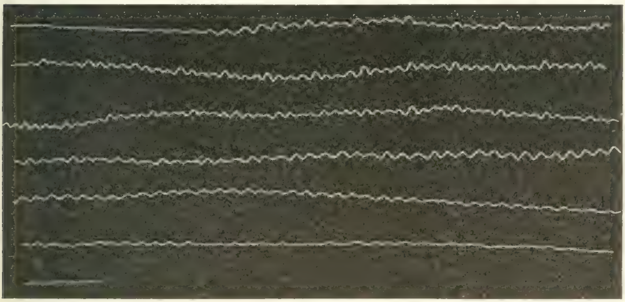


FIG. 61.

Beginning.—The word begins with 7 vibrations belonging to the *th*. These vibrations have a period of 2.5^{σ} or a frequency of 400. These are probably cord vibrations for the same reasons as given in the case of *d* in *die*, p. 39. The amplitude increases rapidly from zero to 0.2^{mm} at the end.

Immediately after the last vibration of *th* there follows the first strong vibration of the set showing the *a* form. The beginning of the *a* is thus prompt and loud.

Pitch.—The successive periods of the cord vibrations in the *ai* are 7.0, 7.0, 7.4, 7.0, 6.7, 6.7 which is maintained with slight fluctuations to the end of the word. The sudden lengthening of the cord period

(that is, the lowering of pitch) at the start is peculiar; it is made specially so because it is accompanied by a sudden rise in the pitch of the resonance tone (see below).

Formation.—The vowel portion of the curve shows throughout its whole length a common character. This character is that of a group of resonance vibrations imposed on each of a series of cord vibrations. In the earlier portion these resonance vibrations are not of equal amplitude while in the later portion they are very nearly so. In the earlier portion there is a strong primary resonance vibration followed by three secondary resonance vibrations (making a total of four resonance vibrations) except in the first two cord periods where there are only two secondaries after the primary (making a total of three). This first portion of the curve resembles that of an *a* but differs in having less difference between the primary and the secondary resonance vibrations; in this fact it resembles the typical *i*.

At the 40th vibration the number of resonance vibrations has changed from four to three, showing a strong initial vibration followed by decreasing ones with a pause before the initial vibration of the next puff. The typical *a* of the preceding examples appears here strongly.

The *i* vibrations may be said to begin in the 44th with three resonance vibrations of almost equal strength, the initial vibration being slightly the stronger.

In the latter portion there are 3 resonance vibrations to every cord vibration; the curve is that of a weak *i* of the kind seen in *eye*, p. 37.

If these vibrations just mentioned, namely the 40th and the 44th, may be considered as limits, the *a* may be said to occupy an interval of 258.3^{σ}, the glide an interval of 19.6 and the *i* an interval of 210.0^{σ}. This subdivision, however, is rather a questionable procedure.

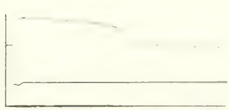


Fig. 62.

...... Resonance tone.

——— Cord tone.

The resonance tone in the first portion begins with a period of 2.1^{σ} or a frequency of 476 which immediately rises to 1.7^{σ} or a frequency of about 588 in the third vibration. The period then changes steadily to 1.9^{σ} at the 40th vibration; it becomes 2.4^{σ} at about the 44th and remains constant to the end. The sudden rise of the resonance tone at the start is accompanied by an equally sudden fall of the cord tone (see above). It seems tnaural to infer that the resonance cavity of the mouth for the *d* must have been lower than that required for the *a*.

There is no trace of a lower resonance tone as described on p. 23.

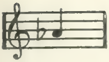


Fig. 63.

Fig. 64.

The changes in the tones of *thy* are sketched in Fig. 62. In general the resonance tone of the *a* can be said to be one of 588 vibrations or approximately as indicated in Fig. 63, and that of the *i* to be of 416 vibrations or as in Fig. 64

Amplitude.—The primary resonance vibration on the first cord vibration of the *ai* has an amplitude of $0.1\frac{1}{2}^{mm}$. Up to about the 50th cord vibration the amplitude fluctuates between 0.1^{mm} and 0.2^{mm}; after that it gradually falls to zero. The fluctuations may be due to interference of the resonance vibrations. The course of amplitude is indicated in Fig. 65 which is a sketch and not a careful plot like Fig. 15.

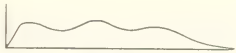


Fig. 65.

Ending.—The word ends by a fall of intensity with maintenance of the cord tension (p. 31).

Relation between curve and color.—The sound of this word *thy* as taken from the record appears to the ear "higher and shorter than the second example; varying more in pitch, rising rapidly at first and then falling" (E. M. C.); "high and short when compared with the second one" (E. W. S.).

The measured results show a shorter word of higher pitch than the second example. There is a slight rise at the start but no fall. The following word *name* is much lower in pitch.

ai in the word *thy* (second example).

The second example of *thy* occurs in the phrase *Thy kingdom come.* A reproduction of the curve is given in Fig. 66. Most of the work on this word has been done by Miss E. M. Comstock.

The curve for this word shows 6 faint vibrations at the beginning. These belong presumably to the *th* and correspond to the strong vibrations of *th* in the first *thy*, and of *d* in *die*. In contrast with the cases just mentioned these vibrations are so weak that little can be said about them definitely except that their period is 2.4^{σ}. It is just possible that they may belong to the first cord vibration of the *a*; this is suggested by the fact that the period is the same as that of the resonance tone of the *a*. Although the matter is doubtful, we have assigned the beginning of the *a* to the end of these vibrations.

The *ai* in this word occupies an interval of 1085^{σ}. It is preceded by a silent interval of 2100^{σ}, represented by a period and including possibly a short time for the *th*. It is followed by a silent interval of 324^{σ} which undoubtedly represents the gutteral *k*.

Pitch.—Begining with a period of 11.9^{σ} the cord tone changes slowly, reaching 8.4 at the 10th vibration, 7.7 at the 20th, 7.4 at the 30th, and 7.0 at the 60th, which it maintains to the end.

Formation.—The curve of the *a* differs from most of the cases of the *ai* studied above in regard to the resonance vibrations. The first resonance vibration for each cord vibration is followed by a second one nearly as strong and this by a third one somewhat weaker, whereas in the previous cases there was one resonance vibration greatly exceeding the rest in amplitude. The curve suggests a more gradual opening

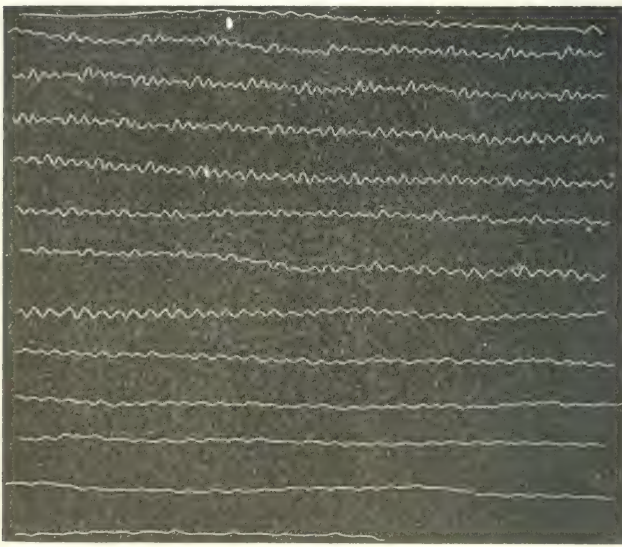


Fig. 66.

of the cords and a less explosive effect; the cord action in this *a* may be supposed to somewhat resemble that in the *i* as explained on p. 37.

There is no strong secondary of the kind described on p. 23. The *a* thus resembles the *a* in *die* and *thy* (above) rather than that in *I* in showing no evidence of a strong lower resonance tone.

The resonance vibration in the first part of the word shows a period of 2.4^{σ}. It rises to 1.6 at the 20th cord vibration falls to 1.9 at the 50th, 2.2 at the 70th, 2.5 at the 90th, and 3.5 at the end. This curious

rise of the resonance vibration during the *a* has not been observed in any of the previous cases. The rise and fall are so gradual that it is impossible to decide on any place as the turning point between them. For the same reason it is impossible to divide the word into *a*, glide and *i*. In the earlier portion the typical *a* form is distinctly seen in the curve and in the latter portion the typical *i* form, but the main portion shows a gradual passage from the former to the latter. There is no sudden increase in amplitude as in nearly all the *i*'s studied.

The changes in the two tones of *ai* are indicated in Fig. 67. It will be noticed that the resonance tone of the *a* begins on the same pitch as the tone of the *d*.

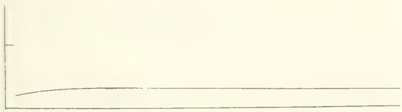


FIG. 67.
. . . . . Resonance tone.
——— Cord tone.

Amplitude.—The amplitude runs from 0.1^{mm} in the first part of the word to 0.2^{mm} at the 30th vibration, falls to 0.1^{mm} at the 50th, increases to 2.5^{mm} at the 70th, maintains this figure to the 80th and gradually falls to zero. The change in amplitude is indicated in Fig. 68.

Ending.—The sound *ai* ends by a fall of amplitude, the respiratory pressure gradually ceasing while the cords are still tense.

Relation between curve and color.—To the ear this word "is longer and more mellow than the first example" (E.M.C.); "begins low and rises with considerable inflection as compared with the first example" (E.W.S.).

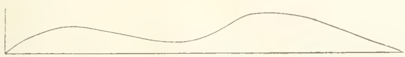


FIG. 68.

The measured results show a very long word, beginning very low and rising in pitch.

General observations on *ai*.

The *ai* in the cases studied above is to be considered as a union of two speech sounds, that is, as a diphthong.

The family of sounds represented by *ai* contains many members that differ greatly in their characters. This is true of the *same speaker on a*

single occasion; the changes for different speakers and for the same speaker on different occasions may be left out of consideration at present.

The first sound in *ai* in words like *fly*, *my*, *thy*, etc., is generally stated to be an *a* (as in *ah*) which inclines toward the mixed *æ*, that is, the vowel sound heard in *burn* and *about*;[1] it may even shade into the palatal *æ* (as in *man*, *fat*)[2] while in some cases it has a tendency to broadening, even to *o* (*not*) as in the Irish.[3] These statements all refer to British forms of pronunciation.

The second sound in *ai* as in *fly* is said to be a very open *i*, something between the *i* in *kin* and the *e* in *ken*.[4]

The diphthong *ai* cannot contain the vowel *ī* as in *keen* or *i* as in *kin*. By holding down the tongue and lower jaw with a pencil it is not possible to pronounce either *keen* or *kin*, whereas there is no difficulty in saying *I*. It seems rather to be the vowel sound heard in the last syllable of *foxes*.

The sounds given above as the British pronunciation of *ai* do not, to my ear, correctly represent the North Atlantic form as heard in the region around New York. In this speech the first sound of *ai* seems to be a somewhat short *a* (as in *father*). Both pronunciation and curve indicate it to be like the *a* in *parson*, below. A similar judgment by the ear has been given by GRANDGENT.[5]

In its first half the North Atlantic *ai* (as in *I*, *eye*) seems to resemble the average German *ai* with a distinct *a* (*father*) sound. The second half seems to be different in the two cases. For the sake of comparison several cases of *ai* were examined in some records which were traced off with the machine described on p. 10 but with a shorter recording lever. Various words like *ein*, *weisser*, *Eis*, *Zeiten*, *Schein*, etc., were closely studied in the tracings from Record No. 1500, *Die Lorelei and Der Fichtenbaum*, by W. L. ELTERICH. When examined under the magnifying glass, the *a* portion of the record showed in most cases curves analogous to those in the cases of *I*, whereas the *i* portion was extremely weak. This peculiarity of the weak *i* in the German *ai* and the very strong *i* in

[1] VIETOR, Elemente d. Phonetik, 3. Aufl., 95, 101, Leipzig 1894.
SWEET, Handbook of Phonetics, [illegible], Oxford 1877.
STORM, Englische Philologie, 2. Aufl., 358, Leipzig 1892.

[2] STORM, Englische Philologie, 2. Aufl., 142, Leipzig 1892.
LLOYD, *Speech sounds: their nature and causation*, Phonet. Studien, 1892 V 263; also a review on p. 87 of the same volume.

[3] SWEET, History of English Sounds, 21, Oxford 1888.

[4] VIETOR, Elemente der Phonetik, 3. Aufl., 95, Leipzig 1894.
STORM, Englische Philologie, 2. Aufl., 103, 358, Leipzig 1892.
LLOYD, Review in Phonet. Studien, 1892 V 87.

[5] GRANDGENT, *English in America*, D. neueren Sprachen, 1895 II 446.

most cases of the American *ai* gives the former the effect of containing a longer *a*. It must be noted, however, that many sounds usually treated as the same are really different. Thus the vowel in *weiss* in *Ich weiss nicht was soll es bedeuten* gives a curve differing greatly in character from that of *weisser* and the other words mentioned above. Again, some of the cases of the American *ai* reported above show a weakening of the *i* that indicates a tendency toward the German form. The details of the work now being done on the German *ai* will appear on a future occasion.

It has been pointed out that the quality of *ai* is different in a strongly accented syllable from what it is in a less accented one, as can be readily heard by comparing the two *ai*'s in *likewise*.[1] This difference is perhaps analogous to that found to exist between *I* and the words *eye*, *die*, *fly*, and *thy*.

The two chief sounds of *ai* are generally said to be joined by a rapid glide, which is not acoustically of much effect except to produce the impression of continuity.[2] Yet it has been asserted that such a diphthong consists in an even and gradual change of the vowel from beginning to end.[3] The above analyses show that the *ai* is not the sum of the two vowels *a* and *i* but an organic union into a new sound *ai*. Thus, there is no necessary pause or sudden change of intensity or change in pitch or even change in character. The later sound shows its influence in the earlier one, and the earlier one keeps its influence far into the later one. This is what would be expected on psychological grounds. The speaker does not think and speak of two sounds separately but of only one; the execution of this one idea by two distinct processes would be unusual. The various degrees of perfection of the synthesis of the two elements would correspond to various expressive characters of the resulting sound.

The degree of synthesis of the two elements would be lessened by any great or sudden change in intensity, pitch or character of the cord tone or the resonance tone. In some of the cases of *ai* there are greater changes than in others.

In so far as they can be considered to be constant, the resonance tones in these cases of the *a* and the *i* were found to be as in Table I. These results may be compared with those of other observers; this is done in Table II.

[1] BELL, Visible Speech, 113, London 1867.

SWEET, Primer of Phonetics, 76, §204, Oxford 1890.

STORM, Englische Philologie, 2. Aufl., 358, 405, 424, Leipzig 1892.

[2] LLOYD, Review in Phonet. Studien, 1892 V 83.

STORM, Englische Philologie, 2. Aufl., 204, Leipzig 1892.

[3] SOAMES, Introduction to Phonetics, 53, London 1891.

TABLE I.

a

	Lower resonance tone.		Upper resonance tone.
I, 1st example	286		1000
I, 2d "	286		1000
I, 3d "	286		1000
I, 4th "	286		1000
[illegible]	385		1000
I, prose example	360		1000
[illegible]	435		1000
My, 1st example			1000
My, 2d "			1000
[illegible]	256		625
Thy, 1st example		588	
Thy, 2d "		416	

i

I, 1st example	450
I, 2d "	555
I, 3d "	500
I, 4th "	400
I, prose "	360
[illegible]	400
Die, 1st example	473
Die, 2d "	473
[illegible]	400
Thy, 1st example	416
Thy, 2d "	288

When allowance for the individualities of different speakers is made, the two resonance tones that I have found for the *a* agree quite well with the tones found by other observers. The serious differences among these observers can be partially explained on the supposition that some have found the lower tone and some the upper one.

Although the *i* in *ai* is not the ordinary long *i*, its resonance tone shows some agreement with those of a few observers. The higher resonance tone noted by other observers was also probably present in the *i* but it was impossible to measure it in the examples studied above (p. 20).

Particular emphasis must be laid on the fact that the tones in a vowel are not constant factors and that the changes they undergo from instant to instant are presumably highly important in producing its peculiar character. Only two previous investigators have observed the change in the cord tone and no one seems to have suspected a possible change in the resonance tone.

TABLE II.

a

	Lower resonance tone.		Upper resonance tone.
a in			
I (E. W. S.)	d^1		b^2
I (E. W. S.)	$f^1\sharp$, g^1		[illegible]
Eye (E. W. S.)	[illegible]		b^2
Die (E. W. S.)			b^2
Fly (E. W. S.)	c^1		[illegible]
Thy (E. W. S.)		d^2	
Thy (E. W. S.)		$g^2\sharp$	
a (WILLIS)		$d^3\flat$, f^3	
a (DONDERS)		[illegible]	
a (HELMHOLTZ)		[illegible]	
a (KOENIG)		b^2	
a (AUERBACH)		a^2, f^2, [illegible]	
a (TRAUTMANN)		f^3, g^3	
a (PIPPING)		$c^3\sharp$–d^3	
a (HERMANN)		f^2, g^2	
a (STORM)		$c^1\sharp$, d^1, $f^1\sharp$, [illegible]	
a (BOEKE)		$f^2\sharp$, c^3	
ai (BOEKE)		b^3	
a (BEVIER)	f^2–$g^2\sharp$		b^2–c^3

i

i in		
I (E. W. S.)		$f^1\sharp$ to [illegible]
Eye (E. W. S.)		$g^1\sharp$
Die (E. W. S.)		[illegible]
Fly (E. W. S.)		$g^1\sharp$
Thy (E. W. S.)		$a^1\flat$, ?
i (DONDERS)		f^3
i (HELMHOLTZ)		f + [illegible]
i (KOENIG)		[illegible]
i (AUERBACH)		c^3, f^1
i (TRAUTMANN)		f^4, g^4
i (PIPPING)		[illegible] [illegible]
i (HERMANN)		$c^4\sharp$–g^4
i (STORM)		d^2
i (LLOYD)		[illegible] + [illegible] [illegible] + [illegible]

The rise of pitch in the cord tone of the vowel *a* has been observed by BOEKE[1] to have extended over more than half a tone in words like *Vader*

[1] BOEKE, *Mikroskopische Phonogrammstudien*, Arch. f. d. ges. Physiol. (Pflüger), 1891 L 301.

(Dutch). MARICHELLE makes the following observations on his phonograph records of the vowel *a* sung on different notes. "The periods corresponding to low tones are divided into two distinct parts; the intensity is feebler in the second half of the period. The gradual modification of the character [timbre] under the influence of variations of pitch operates almost entirely at the expense of the less intense portion of the period; this second half even disappears little by little."[1]

I have observed similar changes in the *a* of the German *ei* and in the vowels *u* and *a* described below.

It seems hardly possible at the present moment to specify the positions of the mouth corresponding to the resonance tones and their changes. Some idea of them may perhaps be obtained in the following way. GRANDGENT'S sections[2] of the mouth for the vowels *a* and *i* are shown in Figs. 69 and 70.

FIG. 69.

FIG. 70.

The following view of the physiological action of the vocal cavities in producing *ai* in the case studied above is proposed tentatively. The depressed position of the tongue for the *a* leaves open a large cavity reaching from the teeth to the vocal cords; the uvula offers no great interruption. The lower resonance tone of the *a* may be considered to arise from the vibration in this cavity. The upper resonance tone of the *a* may be supposed to arise from the rear resonance cavity, that is of the throat cavity from the cords to the slight elevation of the tongue at the uvula. As the *a* changes to *i* this elevation of the tongue moves forward enlarging the rear cavity by including continually more of the mouth; this continuously lowers the upper resonance tone until the tongue comes to rest in the typical *i* position. The variety of changes in the course of the upper resonance tone corresponds to individualities of action of the tongue in the various cases. In some cases the change from *a* to *i* is more sudden and definite (Figs. 14, 21, 27, 44, 62) and in others it is less definite (Figs. 31, 39, 49, 53); in other cases there is even laxity and fluctuation in the typical terminal positions (Fig. 66).

The supposition that the upper resonance tone arises from the cavity

[1] MARICHELLE, La parole d'après le tracé du phonographe, 47, Paris 1897.

[2] GRANDGENT, *Vowel measurements*, Publ. Mod. Lang. Ass., 1890 V 148.

behind the elevation of the tongue rather than from the one in front of it, although opposed to the usual view, does not exclude the presence of tones from the front cavity also. In fact these other tones are presumably also present though not distinguishable in my records.

The greater importance of the rear cavity seems to be indicated by the following facts. The laying of the finger on the tongue does not appreciably modify the enunciation of *a*. When the finger is introduced into the mouth and kept in front of the elevation for the *i*, it produces no appreciable effect: but when it is pushed beyond the elevation into the rear cavity it changes the sound completely.

It may be noted that curious relations exist between the tones of two succeeding sonants (speech sounds with tones): in general it is true that the tones of a sonant form approximately musical intervals with a tone or tones of the preceding sonant.

In all cases of *ai* there is no sudden jump of the cord tone; the *i* continues the cord tone of the *a*, forming with it the easiest musical interval, a unison. This tone is, however, different in different cases; the cord tone of the *a* rises to a certain point selected for that of the *i*. The selection of the pitch of the cord tone for the *i* is influenced by the preceding resonance tones of the *a*, as may be seen in the following table.

	Tone of d, th, l.	*Tones of the a.* Cord, start.	Cord, end.	Lower resonance.	Upper resonance.	*Tones of the i.* Cord.	Resonance.
I, 1st example		56	250	286	1000	250	450
I, 2d "		83	250	286	1000	250	555
I, 3d "		131	250	286	1000	250	500
I, 4th "		111	286	286	1000	286	400
I, prose "		102	180	360	1000	180	360
Eye,		400	160	435	1000	160	476
Die, 1st example	500	179	200		1000	200	473
Die, 2d "	400	217	133		1000	133	473
Fly,	526	160	204	256	625	256	500
Thy, 1st example	400	143	149	588		149	416
Thy, 2d "	417	84	143	416		143	288

In the 1st *I* the cord tone of *i* is practically identical with the lower resonance tone of the *a*; the fixed lower resonance tone of the *a* apparently furnishes a standard toward which the cord tone of the *a* rises to begin the *i*. The cord tone is also just two octaves below the upper resonance tone of the *a*. There is no very simple relation between the resonance tone of the *i* and any of the tones of the *a*.

In the 2d *I* the relations are similar to those in the 1st *I*.

In the 3d *I* the cord tone of the *i* is also practically in unison with the lower resonance tone of the *a* and also at two octaves below the upper resonance tone. The resonance tone of the *i* is just an octave below the upper one of the *a*.

In the 4th *I* the relations are practically as in the previous one except for the fact that the resonance tone of the *i* is two and a-half octaves below the upper resonance tone of the *a*.

In the prose *I* the cord tone of the *i* is an octave below the lower resonance tone of the *a* while the resonance tone of the *i* appears as a continuation of the lower resonance tone of the *a* with no simple relation to its upper resonance tone.

In *eye* the cord tone of the *i* is one and a-half octaves below the lower resonance of the *a* and the resonance tone is practically a continuation of that tone, with no relation to the upper resonance of the *a*.

In *die* (1st example) the cord tone is five octaves below the upper resonance tone of the *a*, which has no lower resonance tone. It is also two and a-half octaves below the tone of the *d*. The resonance tone of the *i* shows no relation to any tones of the *a*, although it approximates the tone of the *d*.

In *die* (2d example) the cord tone of the *a* starts approximately an octave below that of the *d*. No other relations between the various tones are apparent.

In *fly* the lower resonance tone of the *a* is an octave below the tone of the *l*. The cord tone of the *i* in its main portion continues the lower resonance tone.

In *thy* (1st example) the cord tone of the *i* is approximately four octaves below the resonance tone of the *a* and its resonance tone is approximately in unison with the tone of *th*.

In *thy* (2d example) the resonance tone of the *a* is in unison with the tone of *th*. The cord tone of the *i* is three octaves below this tone. The resonance tone of the *i* is an octave above its cord tone and 1½ octaves below the resonance tone of the *a*.

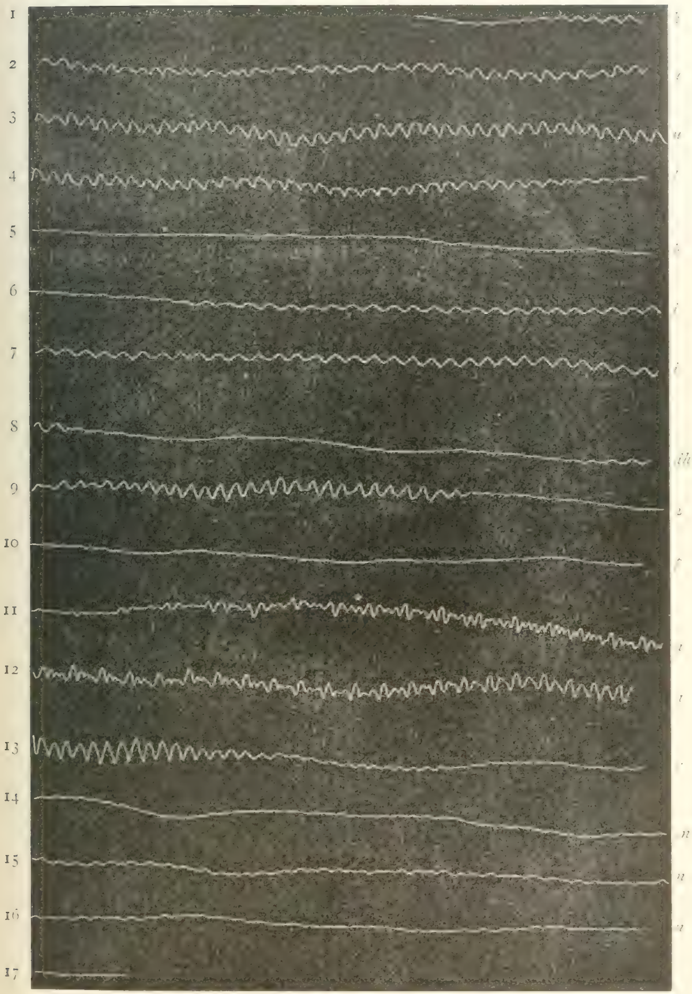
Such a relation between successive tones in speech is merely what would be expected in a melodious voice. An illustration of a similar relation will be found below in the account of the sound *ll* of the word *who'll*.

STUDY OF THE WORDS "*Who'll be the parson?*"

In the following phonetic analysis of a complete phrase I have been much assisted by Miss E. M. COMSTOCK.

The complete curve is given in Fig. 71. It begins with the breath

FIG. 71.



indicated by the letters *wh*; this is not the sound of *wh* in *which* but the breathing *h*.

The aspirate *h*.

The first of the series of sounds is heard as an aspirate followed by the vowel *u*. The curve (Fig. 71, line 1) shows that it occupies a time of 35^σ. Its tone has a constant period of 2.8^σ, or a pitch of about 380 vibrations per second. Its amplitude rises from zero to a maximum of 0.2^{m}. It is thus a "crescendo sustained" sound; in particular, a crescendo sustained light breath. The tone of the *h*, as shown by the vibrations in the curve, is a resonance tone arising from the passage of the air through the mouth; it is not a cord tone. The reasons for considering the vibrations to have arisen from a resonance tone and not from a cord tone are the following: (1) such high cord tones are not found in the other sounds produced by this speaker; (2) the vibrations of 2.5^σ are followed by two vibrations of 2.3^σ and 2.1^σ respectively and then by the vibrations of the *u* beginning with a cord tone of 6.3^σ and a resonance tone of 1.9^σ; the tone of 2.5^σ thus leads rather to the resonance tone of the *u* and could with hardly any possibility be considered as a cord tone with an instantaneous drop of three octaves.

These results do not agree with the view that the first sound of *who'll* is a voiceless form of *u*. The sound *h* is usually said to arise from the breath passing through the mouth already adjusted to the following vowel, the cords being open and the resonance tone alone being heard. "In the opinion of some authorities, *h* has the same position as the beginning of the following vowel."[1] Most later observers have adopted the same view.[2] According to this view we cannot speak of a single *h* but must suppose for each vowel a corresponding *h*: h^a, h^i, h^u, h^e, h^o, etc., each of which has an adjustment of the mouth like that of the corresponding vowel and differs from that vowel only in having a noise from the cords instead of a tone.[3] "Our *h* combines a noise from the cords (and subsidiarly a noise in the mouth-cavity) with the mouth position of a vowel. The common element in h^a, h^i, h^u, etc., does not lie in the vocalic position of the mouth-cavity, which is really different, but in the larynx at the vocal cords, whose position is here a peculiar one, different from

[1] Tâittirîya Prâtiçâkhya, ii 47, ed. by WHITNEY, Journ. Amer. Oriental Soc., 1871 IX 77.

[2] MICHAELIS, *Ueber das H und die verwandten Laute*, Arch. f. d. Studium d. neueren Sprachen (Herrig), 1887 LXXIX 49, 283.

[3] See quotations in MICHAELIS, as before, 79.

that for the loud voice (*vox*) and from that for the whisper voice (*vox clandestina*)."[1]

A special adjustment of the mouth for *h* seems to have been first asserted by VALENTIN who remarks: "The palate, apparently narrowed as a whole, is somewhat drawn upward whereas the root of the tongue is moderately arched."[2] MERKEL asserts: "The whole cavity from larynx to mouth-opening opens or narrows itself at once to the degree required by the following vowel. The tongue however in forming the *h* does not yet assume the position required for the vowel in question. Thus when *i* is to follow it lies lower than the position for this vowel."[3] Both these and a series of later observers apparently supposed the configuration of the mouth to aid in the rough noise of the *h*. This view is undoubtedly partially true as in many cases of *h* the friction of the air can be felt in the mouth. I venture to suggest, however, that the assumption of a particular position for the *h* is for the purpose of giving it a resonance tone instead of making more noise by friction; the curve for *wh* in the case under consideration shows a resonance period of 2.5^σ as contrasted with that of 1.9^σ for the following *u*.

Indications of a tendency to give *h* an independent resonance cavity are apparent in remarks by LLOYD. "*h* and *o* in *hold* are successive, but they slightly overlap. When such a combination is to be produced, the cords instantly leap into a position sufficiently close to cause a slight friction. They then close more slowly, until they are planted close together, and voice ensues. The vowel *position* has already been assumed, but there is no vowel so long as the glottal orifice is still comparatively wide. But there is a moment, just before the cords begin to sound, when the glottis is narrowed to a whispering position; and, for that moment, the sound is both *h* and whispered vowel. If *ho* is whispered, the *h* is still prior, for it begins with a glottal orifice so large as quite to mar the adjusted resonance of the *o* vowel-configuration; and there is no vowel until the close position of whisper is reached. When that is reached, it is held; and the whispered vowel itself may be viewed as the mere promulgation of the final element of the *h*. *h* is therefore really a glide from simple *Mund und Kehlresonanz* (such as is heard in a *sigh*) to a whispered *Anlaut* of the following vowel, *i. e.*, from a nearly uniform beginning to a far from uniform end."[4]

[1] MICHAELIS, as before, 79.

[2] VALENTIN, Lehrbuch der Physiologie des Menschen, II 291, 1844, quoted by MICHAELIS, as before, 61.

[3] MERKEL, Laletik, 72, 1866, quoted by MICHAELIS, as before, 72.

[4] VIETOR, Elemente der Phonetik, 3. Aufl., 22, Leipzig 1894.

There seems to be some conflict between LLOYD's statement that in the *wh* the vowel position has been already assumed and that it starts from a nearly uniform beginning. I would suggest the view that the *h* in this case of *wh* possesses a definite resonance cavity of its own which may be related to but is yet different from that of the following *u*.

The most plausible view of the nature of this *wh* seems to include the following points.

In the first place it is either the glottal fricative produced by a narrowing of the glottal opening sufficient to produce a rough sound without a tone, or a sonant fricative produced by a narrowing of the proper cord glottis while the cartilage glottis remains open.[1] Both views are consistent with the fact that a distinct movement of the larynx can be felt with the fingers when *wh* is pronounced. The former view is consistent with the curve under consideration, but the latter view is favored by some of the other cases of *wh* in the record studied, which show some slight but not quite certain indications of a grouping of the resonance vibrations and therefore of the presence of a cord tone.

The *h* is considered as a sonant in all Sanskirt treatises.[2] Traces of a sonant *h* have been found in speech curves of the Finnish language. The consideration of the vexed question of sonant *h* must be postponed to a future occasion.

In the second place the *h* contains at least one tone arising from the resonance cavity in front of the cords. This tone I believe to be one of a pitch peculiar to *h*, just as certain tones are peculiar to certain vowels. The frequency of the *h* tone in this *h* is 400. I do not believe that for this tone the mouth is adjusted to the position of the following *u* with a resonance tone of 526, and that the pitch of the cavity is modified by the difference in the greater enlargement of the glottal orifice so that the tone 400 is produced. My reasons for this last statement are: 1st, *h* can be sounded alone without giving information concerning the following vowel; 2d, the difference between the opening of the cords for the *h* position and that for the vowel position is too small to produce such a great difference in the pitch of the resonance cavity; 3d, the assumption that the *h* cavity is the same as that of the following vowel is not supported by any positive proof and in the absence of such proof it is unwise to accept an

[1] CZERMAK, *Ueber d. Spiritus asper und lenis, etc.*, Sitz.-Ber. d. Wiener Akad., math.-naturw. Cl., 1866 LII (2) 630, Anmerk. 1 (also in Schriften, I 756).

[2] [illegible] 13.

[3] PIPPING, *Zur Phonetik d. finnischen Sprache; Untersuchungen mit Hensen's Sprachzeichner*, Mém. de la Société finno-ougrienne, XIV, Helsingfors 1899. (Review in Deutsche Litteraturzeitung, 1900, April 28.)

arrangement involving an anticipatory adjustment of the vocal organs whereby the vowel is prepared for before the *h* is produced.

The *hu* glide.

The aspirate *h* is followed by two vibrations with periods of 2.3^σ and 2.1^σ respectively (Fig. 71, line 2). They are resonance vibrations produced by the passage of the air through the mouth cavity. They might with propriety be considered as belonging to the *h*, from which they differ only in period. Yet the change from the *h* period of 2.5^σ denotes the rise of an impulse toward another sound and, if the concept of a glide is to be admitted at all, they are to be treated as a glide. The intention shown in the glide is to change the mouth tone from 2.5^σ for the *h* to 1.9^σ for the *u*. The second of these glide vibrations ends suddenly with the puff of air from the first vibration of the cords in making the *u*.

The vowel *u*.

The word is so short that the ear is not able to attribute any particular quality to the vowel.

The curve for the *u* (Fig. 71, lines 2 and 3) closely resembles that for *ai* in its general character. The first part shows a rising cord tone and a nearly constant but afterwards falling resonance tone. In the latter portion the cord tone is approximately constant while the resonance tone falls. The change in the character of the action of the cords appears clearly also as in *ai* (p. 37). It is, in fact, very evident that this sound is really a diphthong with possibly less difference between the two elements than in the case of *ai*. This diphthongal character of the English *u* is well known to phoneticians; the sound is generally indicated by *uw*. A separation of the sound into its two parts will not be attempted here.

The curve at the beginning of the *u* shows a vibration of 6.3^σ from the vocal cords acting on a cavity whose period 1.9^σ is not a sub-multiple of the cord period. As the cord period is gradually shortened, the resonance period (remaining the same) steadily modifies the form of the resultant vibration, and the curve is seen to change its form gradually. The relation between cord tone and resonance tone is closely analogous to that in the *a* of *ai* (p. 19).

The successive vibrations of the *u* occupy the periods of 6.3, 6.1, 6.1, 5.6, 5.4, 5.4, 4.9, 4.9, 4.9, 4.9, 4.9, 4.6, 4.6, 4.6, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6^σ. The total time occupied by the *u* is 167^σ.

The *u* thus shows a sudden tightening of the cords to a tension necessary for a tone with a period of 6.3^σ and thereafter a gradual increase of

tension to a maximum represented by 4.2^σ, after which there is a fall to 4.6^σ at which the tone remains constant.

The resonance tone begins with period of 1.9^σ or a frequency of 526. For the vowel *u* the following resonance tones have been assigned: Donders, f^1; Helmholtz, f: Koenig, b; Auerbach, g–b, f^1: Trautmann, f^2, g^2: Pipping, $f^{\sharp 2}$–$f^{\sharp 1}$, g^2–b^2: Hermann, c^2–e^2; Storm, a; Boeke, d^4. My measurements indicate a resonance tone of 526 vibrations a second, or approximately c^2. I have not yet been able to settle the question of a lower resonance tone.

This resonance tone is, however, not constant. This is especially evident during the last part of the *u* where the cord tone is constant. In this region of constancy the curve steadily changes its form from the earlier *u* form toward the *l* form; during the last 8 or 10 cord vibrations it is difficult to say whether the curve belongs to the *u* or the *l*. The cord vibrations of the *u* period persist in their own constant period, however, to a point which can be detected. We are thus justified in reckoning these vibrations to the *u* although the mouth cavity has been presumably steadily shaping itself for another sound.

Repeatedly observed facts of this kind have forced upon me the belief that the view of a word as composed of a set of fixed sounds with glides between them is a somewhat inadequate one. It is derived from the attempt to get away from the artificial character of spelling but it still largely retains that character. The usual view of the word *who'll* would represent it as composed of *h*—glide—*u*—glide—*l*. The vocal organs are supposed to occupy three distinct positions, the glides representing the intermediate positions during the moments of change.

A somewhat different view seems better fitted to the actual curves. The unit of speech is sometimes a phrase, sometimes a word, and never a vowel or a consonant unless it is at the same time a word. In speaking a word the vocal organs pass through a series of positions of a special character without stopping in any one position. Thus the word *who'll* represents a continuous change in the force of expiration following a definite plan, also a continuous change in the tension of the vocal cords, likewise continuous movements of the parts of the mouth. The force of expiration rises from 0 to a maximum in 35^σ at the end of the *h*, continues with slight fluctuation during 171^σ in the glide and *u*, and finally dies away at 277 with the end of the *l*. Before the breath begins the mouth has adjusted itself to a tone of a period of 2.8^σ; this position changes very slightly during the 35^σ of *h*; then it makes a rapid change through 2.3, 2.1 to 1.9^σ in the *u*, remains constant during 167^σ and rises suddenly to the mouth tone of the *l* (not determinable here).

On speaking the word *who'll* I perceive apparently *continuous* movements of the lips and tongue; they do not assume fixed positions at any moment. This would agree with the changes just described.

The cord tone has a somewhat similar history. It begins with a period of 6.3^σ in the *u* at 39^σ after the beginning of the word; it rises steadily to 4.2^σ and then falls to a constant pitch of 4.6^σ for the latter part of the *u*; suddenly it rises to 2.1^σ for the *l* and remains practically constant for 71^σ.

There are thus at least three distinct but coöperating continuous processes following different courses throughout the word, namely, the force of expiration, the resonance tone and the cord tone.

It seems thus somewhat artificial to divide the word *who'll* into 3 or 5 sounds; we may preferably say that for the sake of discussion 5 stages in the changing sound may be picked out as typical of the whole process. To illustrate by an analogy, we might take single pictures out of a series of views of a runner made for the kinetoscope and treat the whole movement as made up of a series of positions in which the runner remains at rest. This treatment has its advantages for certain cases but we should never lose sight of the fact that the true movement occurs otherwise.

This view is not inconsistent with the fact that some of the elements of a vocal sound may remain approximately constant for a short time. Thus, the pitch of the *h* is nearly constant—as far as our methods can discover—though the intensity is changing, and the pitch of the *u* is fairly constant for a while.

The liquid *ll*.

The sound *ll* apparently does not begin suddenly but arises from a modification of the *u*. The *u* itself has been steadily changing its character from the very beginning; during its last five or more cord vibrations it gradually approaches the form of curve that characterizes the *ll*. After this point the curve takes the *ll* form which differs completely from that of the *u* at the start (Fig. 71, line 4). As stated above, the explanation is presumably (1) that the cord tone remains on the *u* pitch until a certain moment at which it suddenly rises to the *l* pitch, whereas (2) the mouth cavity begins to modify itself from the *u* form to the *l* form before the cord tone changes. This is quite in agreement with the view that in the English *l* the back part of the tongue is elevated whereby it receives a guttural character[1] and is in this respect related to *u*.

The *l* shows 34 vibrations with a constant period of 2.1^σ. It occupies a total time of 71^σ.

[1] Literature in STORM, Engl. Philologie, 2. Aufl., 139, Leipzig, 1892.

The form of the vibration steadily changes as shown in the figure.

The changes in pitch in this word *who'll* confirm the law deduced for *ai* (p. 57) to the effect that in a succession of sonants (speech elements with tones) the cord tone of a sonant tends to be a multiple or a submultiple of the cord tone or the mouth tone of the preceding sonant.

The relations are not exact but only approximate. The mouth tone 2.5^{σ} of the *h* is followed by a cord tone for the *u* having a general average of 5.0^{σ} or an octave below the former. The mouth tone of the *u* 1.9^{σ} is followed by a cord tone for the *l* of pretty nearly the same period 2.1^{σ}.

Such a law is what would be expected in a voice—at any rate in one that was not unpleasant—for the human ear finds pleasure in a succession of tones whose periods stand in certain relations. Possibly some of the explanation of disagreeable voices may be found in the violation of this law.

In general the curve of this *l* may be said to resemble the forms given by WENDELER[1] and HERMANN and MATTHIAS.[2]

The *l* given by WENDELER is a spoken sound; the figure shows that it must have had a falling cord tone and a decreasing intensity.

The examples of *l* studied by HERMANN and MATTHIAS were sung on notes of different pitch. Their analysis showed that these examples all contained a tone between f^3 and g^3. They also found for the lower notes also a tone that was the octave of the cord tone and changed with it, and for the higher notes a reinforcement of the cord tone itself. This reinforcement of a partial tone of the cord tone is not found in the vowels studied by HERMANN or in the cases of *ai* considered above except in two cases, namely, in the *i* in the 3d example of *I* and in *fly* (see list on p. 57). There is apparently some difference in the action of the mouth in forming the *l*. This difference may be felt by singing the *l* on a note of rapidly rising or falling pitch; there is apparently a movement of the tongue whereby it is pressed more strongly against the palate as the pitch rises. The consequent change in the size of the resonance cavity might, by the appropriate connection between tongue and cord, go parallel with the change in the cord tone and thus always reinforce one of its partials.

Our curve does not enable us to make any measurements of the resonance tones, but its steady change in form while the cord tone re-

[1] WENDELER, *Ein Versuch, die Schallbewegung einiger Consonanten und anderer Geräusche mit dem Hensen'schen Sprachzeichner graphisch darzustellen*, Zt. f. Biol., 1887 XXIII 314, Tafel III, Fig. 21 B.

[2] HERMANN UND MATTHIAS, *Phonophotographische Mittheilungen, V. Die Curven der Consonanten*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LVIII 255, Tafel II.

mains constant shows that the resonance tone or tones change independently. The tongue probably moves while the cords remain at a constant tension. This example of *l* thus differs from those of HERMANN and MATTHIAS.

The labial *b*.

In the spoken words on the gramophone plate the sound *b* follows immediately upon the *ll* without pause. The speech curve at this point (Fig. 71, line 5) shows no measurable vibrations, the enlargement not being great enough to reveal the details of the weak tone of the *b*. The interval occupied is 96^σ.

The vowel *i*.

The vibrations (Fig. 71, lines 6 and 7) have constant period of 2.8^σ. They start with an amplitude of 0 and rises steadily to an amplitude of 0.2^{mm}. At the end they fall to 0 suddenly in four vibrations (Fig. 71, line 8). The pitch of the mouth tone could not be determined. This *i* seems a rather weak vowel when compared with the *i* in *ai*. The sudden ending indicates a quick cut by the following *th* (see above p. 31). The last four vibrations (Fig. 71, line 8) differ somewhat in character from the others and seem to indicate a diphthongal ending to the *i*.

The sonant post-dental *dh*.

As can be heard from the gramophone plate, the *i* sound in *be* is cut short by the *dh* of *the*. This sound appears in the tracing (Fig. 71, line 8) as a space with faint waves following immediately on the sudden fall of the *i* vibrations; the scale of enlargement is not sufficient to give definite information concerning the waves of the *dh*. This sound occupies a time of 56^σ.

The indefinite vowel *ə*.

This vowel follows *dh* in *the*. It rises somewhat rapidly to its maximum, remains at an even amplitude (Fig. 71, line 9), and drops suddenly to 0 in the last 4 vibrations. It has a pitch of 6.7^σ on an average and a maximum amplitude of 0.4^{mm}. The entire vowel contains 12 cord vibrations and occupies a total time of 84^σ.

The *əp* glide.

The vowel *ə* of *the* is cut short by the closing of the lips for *p*. This suddenly reduces the amplitude of the vibrations till they are very faint (Fig. 71, line 9), yet the cords continue to vibrate after the closure as may be seen from the faint vibrations (Fig. 71, lines 9 and 10). The

sound can no longer be considered to be the vowel ə and cannot in the usual sense be called a *p*. It may be treated as a glide although it occupies fully two thirds of the interval of 112^{σ} between the ə in *the* and the *a* in *parson*.

The labial *p*.

If the period of sonancy after *the* is to be considered as a glide, the remaining third of the 112^{σ} may be assigned to the *p* (Fig. 71, line 10).

The vowel *a*.

The word *parson* appears to the ear (E.W.S.) to have an inflectional force of the form indicated in Fig. 72, as often appears at the end of questions: the circumflexion appears to lie in the *a* and the deep fall to be in the *n*; this word seems to contain a trace of an *r*. This word differs

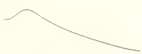


Fig. 72.

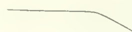


Fig. 73.

from the same word four lines later (p. 15) which appears to the ear to have a deep inflectional tone, at first level and then falling as in deciding a matter; this is indicated in Fig. 73. This latter word seems to contain no *r*. The word *parson* is in both cases apparently continuous with the word *the* and would be acoustically written *theparson*.

The vowel *a* in this case occupies a period of 180^{σ}. It is preceded by the interval of 112^{σ} belonging to the *p* and is followed by a glide of 12.3^{σ}.

It shows 36 cord vibrations. The pitch rises gradually as shown by the following measurements of the successive periods: 6.7, 7.0, 6.7, 6.0, 6.0, 6.3, 5.3, 5.3, 5.3, 5.3, 5.3, 5.3, 4.9, 4.9, 4.6, 4.6, 4.6, 4.6, 4.6, 4.2, 4.2, 4.2, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 4.0, 4.2.

It contains a constant lower resonance tone with a period of 2.8^{σ} or a frequency of 357 (Fig. 35).

The upper resonance tone is one of about 714 vibrations per second.

The amplitude rises through the first four vibrations from zero to 0.3^{mm} and is maintained at this to the end.

The vowel *a* in *parson* has undoubtedly a diphthongal character. The first portion resembles the *a* sound discussed above (p. 16) in the rising cord tone but differs radically in the falling resonance tone, in which respect it is somewhat like the *a* in *die* (Figs. 49 and 53). The latter

portion (Fig. 71, line 13) is related to the earlier portion much as the *i* is related to the *a* in *ai* in respect to amplitude, the lowering of the resonance tone and the continuance of the cord tone. Although this latter portion is not so long as in most cases of *ai*, the resemblance is sufficient to justify the statement with which this paragraph begins. The sound might be written *a*× where the sign × indicates a brief vowel not yet determined. It may be suggested that this brief vowel may arise from the weakening of the *r*, whereby a vowel sound partially or completely replaces the full *r*. It seems, however, to be a general rule, that in English long vowels have a diphthongal character.

The *ar* glide.

The sudden fall in amplitude and the change in pitch of the vowel × in *a*× is continued through an interval of 8.8^{σ} in which 3 vibrations with a period of 2.4^{σ} appear (Fig. 71, line 13, middle). During this time the tongue is presumably passing to the *r* position.

The liquid *r*.

The very brief *r* is distinctly heard in the word *parson;* it occupies a time of 63^{σ} (Fig. 71, line 13 middle to line 14 beginning).

The *r* shows clearly 3 "pseudo-beats"[1] with a period of 19^{σ} or a frequency of 53. The vibrations within the beats are grouped in pairs indicating a cord tone acting upon a resonance cavity. The period of the cord tone is at first constant at 3.5^{σ} (frequency 286) but falls slightly in the third beat. The resonance tone has a period apparently constant at 1.4^{σ} (frequency 714). Still higher resonance tones are probably present. The following explanation of this curve is proposed tentatively. The *r* consists of a cord tone with a frequency of 286 acting upon a resonating cavity adjusted to a frequency of 714. The tongue is adjusted to vibrate with a frequency of 53; this vibration of the tongue closes and opens the air passage so that the intensity of the sound escaping from the mouth is regularly varied from zero to a maximum and again to zero at the rate of 53 times a second.

The pseudo-beats with the cord and resonance vibrations are shown in the curves by WENDELER[2] and in those by NICHOLS and MERRITT. The German rolled *r* of WENDELER has a much longer beat period, in general over 250^{σ} or ¼ sec.; the Finnish *r* of PIPPING has a beat of ½

[1] WENDELER, as before, p. 304.

[2] WENDELER, as before, Tafel II.

[3] NICHOLS AND MERRITT, *The photography of manometric flames*, Physical Review 1898 VII 93, Plates I and II.

to $\frac{1}{2}$ sec.[1] The American rolled *r* of NICHOLS and MERRITT has also apparently a long beat-period as far as can be judged from the pictures. The brief *r* in three examples given by these last observers has apparently a shorter beat-period than that of *parson*. The cord period in WENDELER's examples varies apparently from 2.3^σ to 3.3^σ (WENDELER's own computation of a frequency of 200 or a period of 5^σ can hardly be correct); the resonance period lies in the neighborhood of 1.7^σ, according to my calculation from his records.

The sibilant *s*.

This follows directly upon the *r*. The vibrations in the curve are hardly distinguishable and no very definite limit can be set to them.

The liquid *n*.

This follows immediately on *s* (Fig. 71, line 14 to end). It occupies an interval of 197^σ. The successive vibrations occupy periods of 4.2, 3.5, 5.1, 3.7, 5.3, 4.1, 4.1, 5.3, 4.2, 4.9, 4.9, 5.3, 5.3, 5.3, 5.3, 5.3, 5.6, 5.3, 5.3, 5.6, 5.6, 5.3, 6.7, 6.3, 6.7, 6.7, 7.0, 7.0, 7.0, 7.0, 7.0, 8.4, 8.8, 8.8, 9.1, 8.8. The maximum amplitude is 0.1^{mm}.

IV. The nature of vowels.

To the question, "What is a vowel?" several kinds of answers may be given.

A vowel may be defined as the sound produced by a certain action of the vocal organs. Some specially peculiar position of one or more of the organs is usually selected as characteristic. Nearly every writer on phonetics gives a definition whose elements are the positions of the vocal organs. Such a definition may be called a "physiological definition of a vowel."

Another method of defining a vowel consists in giving the physical character of the sound of which it consists. This method was proposed by WILLIS who justifies it by the following considerations:

"The mouth and its apparatus were constructed for other purposes besides the production of vowels, which appear to be merely an incidental use of it, every part of its structure being adapted to further the first great want of the creature, his nourishment. Besides, the vowels are mere affections of sound, which are not at all beyond the reach of human imitation in many ways, and not inseparably connected with the human organs, although they are most perfectly produced by them; just so,

[1] PIPPING, *Zür Phonetik d. finnischen Sprache*, Mém. de la Soc. finno-ougrienne, XIV, Helsingfors 1899.

musical notes are formed in the larynx in the highest possible purity and perfection, and our best musical instruments offer mere humble imitations of them ; but who ever dreamed of seeking from the larynx an explanation of the laws by which musical notes are governed ? These considerations induced me, upon entering on this investigation, to lay down a different plan of operation ; namely, neglecting entirely the organs of speech, to determine, if possible, by experiments upon the usual accoustical instruments, what forms of cavities or other conditions, are essential to the production of these sounds, after which, by comparing these with the various positions of the human organs, it might be possible, not only to deduce the explanation and reason of their various positions, but to separate those parts and motions which are destined for the performance of their other functions, from those which are immediately peculiar to speech (if such exist).''[1]

WILLIS's idea of studying the physical characteristics of a vowel has been developed by a series of later observers, finding its full expression in the study of curves of speech by the investigators referred to in Section I (p. 2). In its perfection the "physical definition of a vowel" will consist of a mathematical expression for the course of the molecular vibration of the air which it involves.

A third method of defining a vowel might be proposed, namely, a summarization of its mental characters as perceived by the person hearing it. This might be called a "psychological definition." It would consist in a statement of the pitch of the vowel as heard, whereby reference might be made to some standard musical instrument in determining the pitch ; also in a statement concerning its apparent intensity ; also one concerning its apparent length ; and finally one concerning its expressive character. Such definitions have not before been given ; they have been crudely attempted in some cases of the vowels I have studied in the preceding pages.

WILLIS's *theory*.

Probably the earliest well-founded statement in regard to the nature of vowels was that of WILLIS. His line of thought was as follows :

"It is agreed on all hands, that the construction of the organs of speech so far resembles a reed organ-pipe, that the sound is generated by a vibratory apparatus in the larynx, answering to the reed, by which the pitch or the number of vibrations in a given time is determined ; and that this sound is afterwards modified and altered in its quality, by the

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[1] WILLIS, *On vowel sounds, and on reed-organ-pipes*, Trans. Camb. Phil. Soc., 1830 III 231.

cavities of the mouth and nose, which answer to the pipe that organ builders attach to the reed for a similar purpose."

WILLIS fitted a reed to the bottom of a funnel-shaped cavity and obtained sounds resembling vowels by modifying the opening of the cavity. He then tried closed cylindrical tubes of different lengths and found that different vowel-like sounds were produced by different lengths of the tube. His experiments led him to the conclusion that the vowel-like sounds are produced by the repetition of one musical note in such rapid succession as to produce another. "It has been long established, however, that any noise whatever, repeated in such rapid succession at equidistant intervals as to make its individual impulses insensible, will produce a musical note. For instance, let the musical note of the pipe be g'', and that of the reed c', which is 512 beats a second, then their combined effect is $g'' \cdots g'' \cdots g'' \cdots g'' \cdots$ (512 in a second) in such rapid equidistant succession as to produce c', g'' in this case producing the same effect as any other noise, so that we might expect *a priori*, that one idea suggested by this compound sound would be the musical note c'.

"Experiment shows us that the series of effects produced are characterized and distinguished from each other by that quality we call the vowel, and it shows us more, it shows us not only that the pitch of the sound produced is always that of the reed or the primary impulse, but that the vowel produced is always identical for the same value of s [the length of the pipe]. Thus in the example just adduced, g'' is peculiar to the vowel $A°$ [*ȧ* as in all]: when this is repeated 512 times in a second the pitch of the sound is c', and the vowel is $A°$: if by means of another reed applied to the same pipe it were repeated 340 times in a second, the pitch would be f, but the vowel still $A°$. Hence it would appear that the ear in losing consciousness of the pitch of s [the length of the pipe] is yet able to identify it by this vowel quality. But this vowel quality may be detected to a certain degree in simple musical sounds; the high squeaking notes of the organ or violin speak plainly *I*, the deep bass notes *U*, and in running rapidly backwards and forwards through the intermediate notes, we seem to hear the series *U, O, A, E, I, I, E, A, O, U*, etc., so that it would appear as if in simple sounds, that each vowel was inseparable from a peculiar pitch, and that in the compound system of pulses, although its pitch be lost, its vowel quality is strengthened." . . . "Having shown the probability that a given vowel is merely the rapid repetition of its peculiar note, it should follow that if we can produce this rapid repetition in any other way, we may expect to hear vowels. ROBINSON and others had shown that a quill held against a

toothed wheel, would produce a musical note by the rapid equidistant repetition of the snaps of the quill upon the teeth. For the quill I substituted a piece of watch-spring pressed lightly against the teeth of the wheel, so that each snap became the musical note of the spring. The spring being at the same time grasped in a pair of pincers, so as to admit of any alteration in length of the vibrating portion. This system evidently produces a compound sound similar to that of the pipe and the reed, and an alteration in the length of the spring ought therefore to produce the same effect as that of the pipe. In effect the sound produced retains the same pitch as long as the wheel revolves uniformly, but puts on in succession all the vowel qualities, as the effective length of the spring is altered, and that with considerable distinctness, when due allowance is made for the harsh and disagreeable quality of the sound itself."

Thus WILLIS maintains two theses: 1. that a vowel consists of [at least] two tones, a cord tone and a mouth tone; 2. that the mouth tone is independent of the cord tone in regard to pitch.

The first of these theses led to attempts to determine the pitch of the mouth cavity; the results will be considered in Section V below.

The second thesis was for a long time entirely neglected in favor of another one, although, as I hope to show, it is the one that correctly represents the facts.

HELMHOLTZ's *theory*.

According to HELMHOLTZ the vowels arise from the vibrations of the vocal cords through the strengthening of certain overtones by the resonance of the mouth.

"We may well suppose, that in tones of the human larynx, as in those of other reed instruments, the overtones would continuously diminish in intensity with rising pitch, if we could observe them without the resonance of the mouth. In fact they correspond to this assumption fairly well in those vowels that are spoken with widely opened, funnel-like mouth-cavities, as in sharp *A* or *Ä*. This relation is however very materially changed by the resonance in the mouth. The more the mouth-cavity is narrowed by the lips, teeth or tongue, the more prominently its resonance appears for tones of very definite pitch, and by just so much more it thus strengthens those overtones in the tone of the vocal cords which approximate the favored degrees of pitch: and by just so much more the others are weakened."[1]

The pitch of the tones for which the mouth resonates best was studied

[1] HELMHOLTZ, Die Lehre v. d. Tonempfindungen, 4. Aufl., 170, Braunschweig 1877.

by HELMHOLTZ by means of tuning forks held before the mouth. The resonance differed for different vowels.

"The pitch of the strongest resonance of the mouth depends only on the vowel for whose production it has been arranged, and changes essentially even for small changes in the character of the vowel as for example in various dialects of the same language. On the other hand the resonances of the mouth are almost independent of age and sex. I have found in general the same resonances for men, women and children. What is lacking to the childish and female mouth in capacity can be easily replaced by narrower closure of the opening, so that the resonance can still be as deep as in the larger male mouth."

According to HELMHOLTZ "the vowel sounds are different from the sounds of most musical instruments essentially in the fact that the strength of their overtones depends not only on the number of the overtone but above all on its actual pitch. For example, when I sing the vowel *a* or the note E♯, the reinforced tone is b_2, or the 12th one, and when I sing the same vowel on the note b_1 it is the second one."[1]

This view of HELMHOLTZ necessitates the assumption of an accommodation of the resonance tone to the voice tone within quite a range; thus as the voice tone rises or falls the mouth must also change its tone or be able to extend its resonance to a considerable degree. This assumption was made by HELMHOLTZ, the range of accommodation being supposed to extend over as much as an interval of a fifth in music each way from the tone of best resonance. This view has been called the "accommodation theory." According to this theory the mouth must accommodate itself to one overtone of the voice tone and when this rises or falls to a considerable degree it must readjust itself to some other one in order to keep the resonance tone within a limited range.

The difference between the theories of WILLIS and HELMHOLTZ lies chiefly in the relation between the mouth tone and the voice tone; for the former there was no relation, for the latter the resonance tone was one of the overtones of the cord tone.

"WILLIS's description of the acoustic movement in the vowels doubtless coincides closely with the truth; but it gives only the manner in which the motion occurs in the air, and not the corresponding reaction of the ear to this motion. That even such a motion is analyzed by the ear according to the laws of resonance into a series of overtones is shown by the agreement in the analysis of the vocal sound when it is executed and by the resonators."[2]

[1] HELMHOLTZ, as before, 191.

[2] HELMHOLTZ, as before, 191.

HELMHOLTZ also devised an apparatus of electric tuning forks and produced vowel-*like* sounds by combining a fundamental tone with different sets of overtones.

HELMHOLTZ was greatly influenced in his theory by his views of the action of the ear. The hypothesis that all regular vibratory movements reaching the ear are analyzed by it into a series of harmonics of the fundamental period is an assumption that seems to lead necessarily to the HELMHOLTZ theory. This assumption, however, we must disregard at the present time; the problem concerns the nature of the vibratory movement characterizing a vowel and the solution must be found in an unbiased analysis of the vowel curve; the question of how the ear acts is a later one.

PIPPING'S[1] work with HENSEN'S instrument (see above, p. 4) led him to the following conclusions.

"In agreement with HELMHOLTZ I have found that each vowel is distinguished by one or more regions of reinforcement of constant pitch. The intensity of its partial tone is, *cæteris paribus*, greater as it coincides more accurately with the range of reinforcement.

"In regard to the range of the reinforcement I cannot agree with HELMHOLTZ. HELMHOLTZ indeed states that the range can be different according to the opening of the mouth, the firmness of walls of the oral cavity, etc. But he lays so little weight on this difference that he does not attempt to use it in the characterization of the different vowels. To judge from page 183 of the Lehre von den Tonempfindungen HELMHOLTZ thinks that the range of reinforcement must extend in general at least a musical fifth above and below, and this is certainly not the case.

"Sung vowels contain only harmonic partial tones." That is, a vowel produced by singing consists of a series of tones whose vibrations stand in the relations of 1 : 2 : 3 : 4 : ⋯.

"The intensities of the various partial tones do not depend to any essential degree on their ordinal numbers." That is, in distinction to most musical instruments it is not the fact that the first partial is much the stronger and that the higher partials are in general weaker.

"The various vowels differ from each other in ranges of reinforcement which are of different numbers, width and position in the scale of pitch." That is, one vowel may have two ranges of reinforcement, another three, etc., and these ranges may differ.

On a later occasion[2] PIPPING believes that the range of accommodation may exceed even the limits allowed by HELMHOLTZ.

[1] PIPPING, *Zur Klangfarbe der gesungenen Vocale*, Zt. f. Biologie, 1890 XXVII 77.

[2] PIPPING, *Zur Lehre von den Vokalklängen*, Zt. f. Biologie, 1895 XXXI 573, 583.

Comparison of the two theories.

The two conflicting theories require a decision concerning their validity.

Among the results that support the view of WILLIS we may notice those obtained by DONDERS with the SCOTT phonautograph.[1]

"Each of the fourteen vowels when sung on a constant tone produces a constant curve. . . . "For each vowel the form of the curve changes with the pitch. This result is connected with the peculiarity of the vowels, that their timbre is determined not by overtones of a certain order to the fundamental, but rather by overtones of a nearly constant pitch."

This last statement implies the fact that if the resonance tones of the mouth were overtones of the voice tone bearing a definite relation to it, such as 1st, 2d, the curve would remain the same in form no matter what the pitch, just as the curve of vibration for a violin string has a typical form which persists in spite of changes in the pitch of the string. On the other hand if the tone of the mouth is a constant one, as WILLIS assumes, the combined vibration produced by the voice tone and the mouth tone would change for any change in pitch of the voice tone.

HERMANN's investigations were carried out by transcribing the curves of song from the phonograph. He finds that the essential fact in a vowel is the intermittent or oscillatory blowing of the mouth tone by the voice. Under such circumstances it makes no difference whether the resonance tone coincides with any fraction of the voice tone period or not.[2] HERMANN thus supports the theory of WILLIS in asserting that the mouth tone is completely independent of the voice tone. To this statement HERMANN adds that of the intermittence of the voice tone which seems never to have been suspected by previous observers. This new fact of intermittence appears much more clearly in my curves of the spoken *a* (see Figs. 7, 17, 30) than it does in HERMANN's curves of the sung vowels. HERMANN believes that this intermittence is essential to the production of a vowel and that merely adding a constant tone to a complex of tones does not give a vowel.[3] This intermittence, however, occurs only in some vowels of low pitch, as in the first portions of the cases of *a* just mentioned; it does not occur in the *i*. Even in the latter portion of my

[1] DONDERS, *Zur Klangfarbe der Vocale*, Annalen de Physik u. Chemie, 1864 CXXIII 528.

[2] HERMANN, *Phonophotographische Untersuchungen*, Archiv f. d. ges. Physiol. (Pflüger), 1890 LXXIV 380, 381.

[3] HERMANN, *Weitere Untersuchungen ü. d. Wesen der Vocale*, Archiv f. d. ges. Physiol. (Pflüger), 1895 LXI 192.

cases of *a* it is hardly proper to speak of intermittence; the pressure in the wave from the voice tone is not evenly distributed throughout the period, but there is nothing resembling intermittence. Even in HERMANN's own curves for *i* as shown, for example, in one of his latest publications,[1] there is no such intermittence.

According to HERMANN each vowel has one or two fixed mouth tones whose pitch varies within narrow limits if at all; these tones he calls "Formants." Thus, the vowel *u* when sung by a certain person contains not only the voice tone but also one or two mouth tones; these mouth tones are the same when the same vowel is sung at different pitches.

HERMANN has objected to the overtone theory of the mouth tone that in many voices the formant is so high above the voice tone that it cannot be supposed that an overtone of that pitch could possibly be present. Thus as the voice-tone *G* the vowel *i* has a strong mouth tone that would correspond to the 28th or 29th partial of the voice tone, whereas such a high partial, if present at all, would be too weak to be heard.[2]

A final decision in the case of the vowel *a* can, I believe, be established on the basis of the curves described above in Section I. The independent tone theory is certainly the only one that will account for this vowel. In the first place the vowels studied were spoken vowels and were open to none of the objections that may be made against sung vowels. In the second place the resonance vibrations can be seen starting at regular intervals and dying away completely in some instances and less completely in others within a single period of a voice tone. Again, the resonance vibration can be seen to remain of constant period while the voice tone rises through a distance of several octaves *within one single vowel.*

In the face of such conclusive evidence it is hard to see any point in which the decision in favor of the theory proposed by WILLIS and developed by HERMANN can possibly be attacked. It is natural to assume that a theory found to be valid for one vowel will be valid for all; it is, of course, possible that other laws may hold good in other vowels, but until this possibility is proven we can treat all vowels on the independent-tone theory.

The noise theory.

Another view of the way in which the resonance tone is aroused resembles an older view of the action of organ pipes. "The concomitant resonances [mouth tones] which create or constitute vowel quality are

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[1] HERMANN, *Weitere Untersuchungen über d. Wesen der Vocale*, Archiv f. d. ges. Physiol. (Pflüger), 1895 LXI Tafel V.

[2] HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LVIII 274.

animated, primarily and essentially, by the irregular noises which issue, *together* with the vocal tone from a speaking or singing glottis, but *without* it from a whispering one. Some of these are always found capable of affording just the appropriate impulse, and of kindling the resonances of the configuration [mouth cavity]."[1] This view is undoubtedly correct as far as whispered vowels are concerned, but it can hardly be supported for spoken vowels. In one respect the case is analogous to that of an ordinary resonator; by blowing against the opening or by tapping the walls the tone of the resonator can be faintly heard. Thus, in whispering, the vowels can be produced with faint tones. These faint tones are, however, quite different affairs from the strong mouth tones of spoken vowels although they may be of the same pitch. In speaking there must be a stronger force to set the mouth cavity in vibration than the faint noises that accompany the cord tone; otherwise the mouth tone would be quite overpowered by the cord tone and there would be no noticeable difference between vowels spoken on the same note. Moreover, noises seem to have no power to arouse strong resonances; thus the noise of *s*, though loud and produced directly on the edge of the resonance cavity, does not produce any marked resonance vibrations (p. 70). The force that sets the mouth cavity in vibration can only come from the cord tone and the "noise theory" of vowels may be definitely laid aside.

Observations on the nature of spoken vowels.

Previous investigators have had in mind almost exclusively the vowels sung on musical notes. It has been universally assumed that the spoken vowels do not differ essentially from the sung ones. Thus HERMANN says, "The difference between sung and spoken articulation lies exclusively in the fact that the pitch, intensity and duration of the syllables—or more accurately, of the vowels—are governed in song by melody and rhythm and in speech by the laws of emphasis according to meaning and arrangement. In a single vowel there can thus be absolutely no difference between song and speech."[2]

My investigations show, I believe, that this view is erroneous.

In the first place the voice tones of spoken vowels are seldom of constant pitch. Some are nearly constant in pitch, some fluctuate, some rise and fall in various simple or complicated ways. I have looked over hundreds of vowels in the records and find that there is a typical tone for the whole discourse which occurs in a majority of the vowels, while the others

[1] LLOYD, *Speech sounds: their nature and causation*, Phonet Stud., 1890 III 277.

[2] HERMANN UND MATTHIAS, *Phonophotographische Untersuchungen*, Archiv f. d. ges. Physiol. (Pflüger), 1894 LVIII 258.

have quite different tones. Many of the vowels are fairly constant, but many others vary. Indeed, it is just such changes and fluctuations in pitch and also in intensity that enable the voice to express the character of the thought. Without these changes the speech would be a monotonous sing-song resembling the speech of the deaf who have been taught by the oral method. When words are sung, they lose most of their character; speech is capable of expressing by its modulations the various emotions and conditions of the individual, whereas the singer has few resources at his command.

In the second place vowels have certain characteristic laws of pitch and intensity in certain positions. Thus the *a* of *ai* in my curves begins practically at zero in both pitch and intensity. The *i* has a nearly constant pitch with a slight fall, and a peculiar rise and fall of intensity. Presumably we shall at some time be able to determine the analytical expressions for the vowels and shall find that their properties follow definite laws.

It is interesting to note that this change in pitch in the spoken vowels has so generally escaped notice. I know of only one recorded observation that might refer to the subject.

ARISTOXENUS,[1] in discussing *κίνησις φωνῆς* opposes *κίνησις συνεχής* to *κίνησις διαστηματική*. The first term may be translated as "change in pitch of the voice," the second as "continuous change," and the last as "change by steps." The continuous change he considers to be characteristic of speech as opposed to song. "Now the continuous movement is, we assert, the movement of conversational speech, for when we converse the voice moves through a space in such a manner as to seem to rest nowhere."[2] It is not quite clear to me what he means by "continuous change." If he had definitely in mind the change in pitch of a vowel within itself, he certainly furnishes an example of most precise hearing and careful observation whereby he anticipates a result arrived at later only by careful experimental methods. I am somewhat inclined to doubt that he had in mind anything more than the general observation that in speech the voice rises and falls irregularly, yet the special statement that the changes are *continuous* necessarily involves the changes within single vowels.

One of the most curious facts observed in the vowels studied in the previous section is the change of the resonance tone. The pitch of the

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[1] ARISTOXENUS, Harmonica, I § 25, p. 8, Meib. The passages are collected in JOHNSON, *Musical pitch and the measurement of intervals*, Thesis, Baltimore 1896.

[2] ARISTOXENUS, Harmonica, I § 28, p. 8, Meib., quoted in JOHNSON, *The motion of the voice in the theory of ancient music*, Trans. Amer. Philol. Assoc., 1899 XXX 47.

resonance tone is frequently not a fixed one but one altered according to some law. In most of the cases of the *a* it begins to change in the latter portion ; in the *i* it is frequently constant but often falling.

To the foregoing account of vowels it is necessary to make some additions. The most important one is the statement that a vowel is not a fixed thing, but a changing phenomenon. There is no such thing as a vowel *a* with a definite character under all circumstances. Even for the same speaker there are continual changes and variations in this vowel. For different speakers, for different dialects and for different languages the changes become so great that the *a* finally has little resemblance to the one chosen as a standard. We may say that a large number of our speech sounds may be classed together by a more or less close resemblance and may be designated by the term *a*. A similar statement would hold good of any speech sound.

The changes from *a* take place in all directions, in voice tone, in mouth tone, in length, etc. By selecting examples properly a continuous series can be made of forms whose members differing but little from their neighbors, reaching from *a* to any of the other vowels. For example, between a typical *a* and a typical *o* all the intermediate vowels may be found corresponding to the position of the mouth between the *a* position and the *o* position. "In no language or dialect are the sounds which pass current for one and the same vowel absolutely identical. They vary perceptibly in individual use : and hence . . . a vowel is not one single definite sound, but a group of more or less closely resembling sounds which in a given speaking commuity pass current as one vowel. There seems to be no practical limit to the range of this wandering so long as the sounds employed do not actually *overlap* those of any other vowel which happens to be used in the same language."[1]

Mechanical action in producing vowels.

Although it may be regarded as settled that a vowel consists of a cord tone with its overtones and one or more resonance tones from the mouth and possibly from the pharynx,[2] there still remains the physical problem of the method in which the cord tone arouses the resonance tone.

The mouth cavity with the pharynx and vocal cords may be considered as a pipe with membranous reeds. The theory of its action will be similar to that of an ordinary reed organ pipe.

Each vibration of the reed sent a wave of condensation and rarefaction along the pipe. When the pipe is of such a length that this wave is re-

[1] LLOYD, *Speech sounds : Their nature and causation*, Phonet. Stud., 1890 III 254.
[2] LLOYD, brief note in Proc. Brit. Assoc. Adv. Sci., 1891 796.

flected back in such a way as to reinforce the vibration of the reed, the resonance tone is a loud one. Thus, when a properly adjusted resonator is placed behind a vibrating fork the tone of the fork is strongly reinforced. The reinforcement is also strong when the resonator coincides in pitch with an overtone of the reed.

Such a coincidence between the periods of the pipe tone and the reed tone is not necessary. Each impulse from the reed may be considered as striking the pipe with something of the nature of a blow, whereby the proper tone of the pipe itself may be aroused for an instant. The pipe may thus have its own pitch and be heard, no matter what relation there may be between it and the pitch of the reed. When the blow from the reed is rapidly repeated, both the reed tone and the pipe tone will be heard.

Such a method of producing resonance tones has been declared to be impossible by HENSEN,[1] who remarks that air from a reed pipe cannot arouse a resonance tone. The experiment on which he bases this statement consisted in placing a resonator at the end of a reed pipe. At a certain pressure of air the pipe sounded its own tone, at a different pressure it was silent. The resonator sounded only when the pipe was silent. Nevertheless there were occasions when both the pipe tone and the resonance tone appeared together: these were called by HENSEN unsuccessful experiments. We ought perhaps to call them rather the successful ones.

To these experiments and deductions HERMANN replied that a labial pipe can be used to sound a reed pipe, and some experiments were made to demonstrate the fact.[2] I have attempted in another way to show that a series of puffs of air of any periodicity may be used to sound a labial pipe of any pitch.

A disc with its edge cut into waves forming approximately a sine-curve was rotated by an electric motor at any desired speed. Its edges passed between the ends of two pieces of rubber tubing so arranged that the air blown into one of them passed directly into the other one if the waves of the disc permitted; the position was so chosen that the waves of the disc regularly interrupted the air current completely. The end of the rubber tubing was flattened and placed so as to blow against the edge of a piece of brass pipe stopped at the other end. The experiment began with the disc at rest. A current of air was blown through the tubing; the pipe gave forth a tone. The disc was then set in rotation; the tone

[1] HENSEN, *Die Harmonie in den Vocalen*, Zt. f. Biol., 1891 XXVIII 39.

[2] HERMANN, *Weitere Untersuchungen ü. d. Wesen d. Vocale*, Arch. f. d. ges. Physiol. (Pflüger), 1895 LXI 195.

of the pipe was regularly intermitted. As the disc moved faster, this intermittence became more rapid. Finally, the intermittence itself was heard as a tone in addition to the pipe tone. Thus an intermittent air current, such as is employed for producing tones directly, can be used to produce a pipe tone in addition.

I have even succeeded in arousing the resonance of a closed tube by blowing through an artificial larynx. The artificial larynx was made by binding a piece of thin soft rubber around the end of a glass tube. Two opposite points of the thin-walled rubber tube thus made were each caught between the thumb and finger; the tube was then stretched till the sides come together. A blast of air through the tube set these edges in vibration and produced a tone. By placing the edges at the right spot over the mouth of a bottle or a test-tube or a key (Fig. 74) the resonance tone of the latter could be distinctly heard.

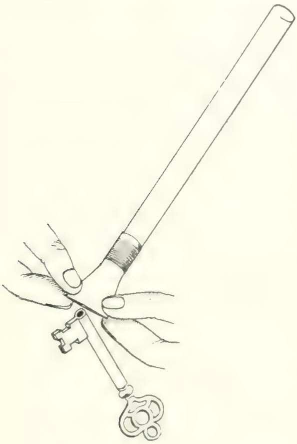

FIG. 74.

When the edges of the artificial larynx are properly placed against the opening of a small tube such as the hole of a door-key, the tone of the key is heard loudly in addition to that of the artificial larynx. The pitch of the larynx tone may be altered at will. This experiment illustrates with great vividness the method in which vowels are actually produced in the vocal organs.

It is not so easy to arouse a tube of low pitch such as a bottle in this way, because the volume of air passing through the artificial larynx is not large.

It can thus be regarded as definitely settled that the current of air from a reed can be used to arouse a resonance tone in a cavity properly adjusted to receive the air. The WILLIS theory of vowel production is therefore at least a physical possibility.

To this statement we may add that the reed tone and the resonance tone may vary independently of each other, but that the resonance tone is loudest when its pitch is higher than that of the reed tone.

WILLIS's view of the way in which the resonance tone was superimposed on the reed tone is very explicit. "According to EULER, if a single

pulsation be excited at the bottom of a tube closed at one end, it will travel to the mouth of this tube with the velocity of sound. Here an echo of the pulsation will be formed which will run back again, be reflected from the bottom of the tube, and again present itself at the mouth where a new echo will be produced, and so on in succession till the motion is destroyed by friction and imperfect reflection. . . . The effect therefore will be a propagation from the mouth of the tube of a succession of equidistant pulsations alternately condensed and rarefied, at intervals corresponding to the time required for the pulse to travel down the tube and back again ; that is to say, a short burst of the musical note corresponding to a stopped pipe of the length in question, will be produced."[1]

The true view of the action of the mouth in producing a resonance tone seems to be the following one. The sudden puff of air from an explosive opening of the cords may be considered to act as a piston compressing the air before it in the mouth cavity. The air acts as a spring by its resistence to compression and drives the piston back beyond its position of equilibrium ; the resistance to dilatation draws it back, and so a vibratory movement is set up. Under these circumstances the air acts merely as a spring ; the form of the cavity is immaterial and the period of vibration remains the same, provided the capacity be not varied. The single impulse of the piston thus makes the resonator a source of vibration, whose period remains practically constant but whose amplitude steadily diminishes from loss of energy mainly by communication to the external air. Such vibrations are seen in the curves for *a* in Section II above. This statement is an adaptation of that given by RAYLEIGH for resonators in general.

The question arises as to the period of the tone thus produced by the resonator.

There are cases in which the HELMHOLTZ view of the action of the mouth cavity might seem to have a possibility of correctness. If we assume (1) that a uniform condition has been attained, (2) that the natural period of the resonator does not differ greatly from that of the cord period, and (3) that the cord vibrations are of not too explosive a nature, it follows that the effect of the resonator can only be to modify the intensity and phase of the partials of the cord note. The partial or partials nearest to the natural periods of the mouth cavity will be reinforced and they can be found from the cord by the FOURIER analysis.

Under the assumptions made above the vibration of the resonance cavity is a *forced* one, and the conclusion concerning the section of the

[1] WILLIS, as before, 243.

mouth cavity is necessarily correct.[1] The first and second assumptions made above have been explicitly stated by RAYLEIGH, who concludes that both the WILLIS and the HELMHOLTZ ways of treating the action of the mouth cavity are legitimate and not inconsistent. "When the relative pitch of the mouth tone is low, so that, for example, the partial of the larynx note most reinforced is the second or the third, the analysis by FOURIER'S series is the proper treatment. But when the pitch of the mouth tone is high, and each succession of vibrations occupies only a small fraction of the complete period, we may agree with HERMANN that the resolution by FOURIER'S series is unnatural, and that we may do better to concentrate our attention upon the actual form of the curve by which the complete vibration is expressed."[2] The two forms of treatment imply that the resonance tone is to be considered in the one case as a free vibration of the air in the cavity, and in the other case as a forced vibration. Some cases of the *i* (Figs. 44 and 53) may be reconciled with the HELMHOLTZ view, the resonance tone being an overtone of the cord tone and changing with it. The cases of *a* and most of those of *i* are decidedly inconsistent with the overtone theory. Possibly the variation from the overtone theory arises from the explosive manner in which the cords open. The general description of their action for *a* probably holds good even when the resonance tone is only about an octave above the cord tone; each puff of air is stronger at the start and fades away, setting the air in the resonance chamber into free instead of forced vibration. This general characteristic can be traced in each *a* even to the point where the resonance tone is slightly less than the octave of the cord tone, as in Fig. 11. We are probably justified in concluding that the WILLIS theory of the production of vowels holds good universally.

V. THE MOUTH TONE IN VOWELS.

DONDERS sought to determine these tones by noting the pitch of the mouth cavity when the various vowels were whispered.[3]

HELMHOLTZ[4] and AUERBACH[5] by holding tuning forks before the mouth when it had been fixed for a certain vowel have found those whose tones are most strongly reinforced. The mouth acts as a resonator and the tone most strongly reinforced is that to which the mouth is tuned. The

[1] RAYLEIGH, Theory of Sound, § 48, 66, 322k, 397, London; 1894, 1896.

[2] RAYLEIGH, as before, § 397.

[3] DONDERS, *Ueber d. Natur. d. Vokale*, Archiv f. d. holländ. Beiträge z. Natur. u. Heilkunde, 1858 I 157.

[4] HELMHOLTZ, Lehre v. d. Tonempfindungen, 4. Aufl., 171, Braunschweig 1877.

[5] AUERBACH, *Untersuchungen ü. d. Natur. des Vokalklanges*, Diss., Berlin 1876.

objections arise: that there is no certainty that the mouth is really in the vowel position desired; and that the mouth may resonate to several tones. The adjustment of the mouth may be quite different when no expiration is occurring from what it is during whispering or speaking or singing.[1] At any rate we have no assurance that it is the same. I quite agree with HERMANN that the only trustworthy determinations of the mouth tone are those obtained by actual whispering, singing or speaking. Whispered vowels were examined by DONDERS, HELMHOLTZ and HERMANN.

The pitch of the mouth tone has been studied in a different way by LLOYD. The mouth, as an excentric cavity, would naturally have two resonance tones: the tone of the "porch" or narrow front part, and the tone of the "chamber" or rear part.[2] A combination of a tube and a cylinder can be made to give a vowel-like sound when the sizes are properly selected. LLOYD produced various vowel-like sounds and determined the tones of the tube and the cylinder. The vowel-character of a sound is, according to LLOYD, essentially determined by the relations of pitch between these two tones, or among several tones when there are more than two.

LLOYD[3] has also mapped out the forms of the mouth cavity involved in different vowels and has calculated the tones to which they would resonate. Thus for the vowels in the following words he has calculated the resonance tones as indicated: *piece* 2816, *pit* 2500, *rein* 2112, *there* 1508, *man* 1431, *half* 1082, *law* 834, *note* 623–444, *put* 528, *blue* 314.

Another method used in seeking the mouth-tone consists in analyzing the curve of vibration representing the vowel into a series of curves representing simple tones and determining which of these tones above the voice tone is apparently the loudest.

A simple tone is defined as one for which the deviation of the material particle from its position of rest is given by an expression of the form

$$y = a \sin \frac{2\pi t}{T}$$

where y is the deviation at the moment t, a the amplitude or maximum value of y, and T the time of one complete vibration of the particle through its positive and negative phases. A curve of this kind is called a "sinu-

[1] HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 374.

[2] LLOYD, *Speech sounds; their nature and causation*, Phonetische Studien, 1890 III 275, 278; 1890 IV 39; 1891 V 125.

[3] LLOYD, Proc. Roy. Soc. Edin., March 1898.

soid" or a "harmonic" and such a vibration is said to be sinusoidal or harmonic. The exact expression for such a vibration must give the phase from which the values of t are measured; this is done in

$$y = a \sin\left(\frac{2\pi t}{T} - \varepsilon\right)$$

where ε indicates the time between $t = 0$ and the next preceding moment when $y = 0$.

A number r of sinusoids superimposed give a vibratory movement in which

$$y = \sum_{n=1}^{n=r} A_n \sin\left(\frac{2\pi nt}{T} - \varepsilon_n\right).$$

It can be proven that any single-valued finite periodic function with the period T can be expressed by a series of sinusoids whose periods are $T, T/2, T/3 \cdots$. This is generally known as FOURIER's theorem.[1] The analysis of such a function into a series of sinusoids is known as the FOURIER analysis.

Likewise a number of sinusoids may be added to produce a vibration resembling some given curve. Such a synthesis can be performed by machines constructed for the purpose, for example, the machine of PREECE and STROH[2] or that of MICHELSON.[3] The curves produced by PREECE and STROH somewhat *resemble* the curves of vowels, but so distantly that they indicate the impropriety of considering a vowel curve as a sum of a series of harmonics.

A vowel curve gives by the FOURIER analysis a series of sinusoids of various amplitudes.[4] Those of greatest amplitude are assumed to be the most prominent tones in the complex tone of the vowel. It is also assumed that the one or more stronger tones after the fundamental are the tones of the mouth.

As an objection to this method we are entitled to say, that the FOURIER analysis is in this case a means of representing a vibratory movement by a formula. We may add that it is nothing more than an interpolation formula by which the value of y can be found for any desired instant of

[1] FOURIER, Theorie analytique de la chaleur, Ch. III, Paris 1822.

[2] PREECE AND STROH, *Studies in acoustics. I. On the synthetic examination of vowel sounds*, Proc. Roy. Soc. London, 1879 XXVIII 358.

[3] MICHELSON, *A new harmonic analyzer*, Amer. Jour. Sci., 1898 (4) V 1.

[4] The scheme for the computation and various essential practical devices are given by HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 47.

time. It is merely one case of a more general method[1] of interpolation by a periodic series; it is thus considered in works on the adjustment of measurements.[2]

Such an interpolation formula remains simply a mathematical tool unless it is found to express the actual nature of the phenomenon measured. It has been assumed by practically all writers, that all musical sounds are really combinations of a series of sinusoidal partial tones: for example, it can be readily demonstrated that a violin string vibrates not only as a whole, but also in halves, thirds, quarters, etc. It is also presumably true that each of these parts produces a sinusoidal vibration of the air. Thus, the peculiar tone of the violin is presumably really the sum of a series of approximately sinusoidal tones. The FOURIER analysis in such a case undoubtedly expresses the nature of the tone.

In the case of sung vowels the assumption that the vocal cords vibrate like reeds, and the further assumption that the mouth acts as a resonator reinforcing one or more of the partial tones of the cord would justify the use of the FOURIER analysis for finding the partial tones of the voice-tone and also the tones reinforced by the mouth, provided these assumptions were proved to be correct.

The vocal cords are certainly to be treated as membranous reeds. In the main their vibrations can be supposed to follow the usual laws.

The other assumption, that the mouth acts also as a resonator to reinforce some of the partial tones of the cord vibration, is certainly not justified (p. 73). The main effect of the mouth is to impose a vibration of its own upon the vibration coming from the cord. The reinforcement of partial tones may possibly be present, but it is certainly not prominent. The FOURIER analysis would be applicable only if the mouth tone were coincident with one of the partial tones of the voice tone; this is, at least generally, not the case in song, as has been indicated by WILLIS, DONDERS and HERMANN, and is certainly not the case in speech as is proven by my curves for *a*. With a mouth tone not coincident with a partial tone the FOURIER analysis may, in a vowel of constant pitch, indicate a reinforcement of the nearest partial vibration, or it may show reinforcement of the two nearest partials above or below. The analysis can thus be used to indicate the approximate pitch of the mouth tone in such a case, although it may not coincide with a partial of the voice tone.[3]

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[1] GAUSS, *Theoria interpolationis methodo nova tractata*, Werke III 265, 1876.

[2] WEINSTEIN, Physikalische Maassbestimmungen, I 486, Berlin 1886.

[3] HERMANN, *Phonophotographische Untersuchungen*, Archiv f. d. ges. Physiol. (Pflüger), 1894 LVIII 276.

With vowels of changing pitch, as in my examples of *a*, any attempt to apply the FOURIER analysis would be an absurdity. In this vowel the pitch of the voice tone changes from vibration to vibration. The analysis would be thus utterly different for each vibration and would indicate a different mouth tone every time, whereas the reonance vibrations can be seen in the curves to remain constant.

It is an imaginable hypothesis that, since the period of the voice tone in a rising or a falling vowel is not the constant T but some value $f(t)$ which steadily changes, we might make an analysis into a series of sinusoids whose periods change likewise. We would thus have

$$x = \sum_{n=1}^{\infty} A_n \sin\left(\frac{2\pi n t}{f(t)} - \epsilon_n\right).$$

The expression for $f(t)$ would differ for different vowels. Such an analysis might accurately represent the case when a musical sound composed of a fundamental with overtones is reproduced on a phonograph whose speed is constantly accelerated. It might also be applicable to the analysis of a glide produced on a musical instrument like a violin. The curve, however, would be of the same form in each period, which—as DONDERS first pointed out and I have abundantly shown—is not the case in the vowels.

Other methods of finding the pitch of the mouth tone may be used. The method that suggests itself at once is simply that of measuring the length of a wave of the mouth tone. This could best be done in my curves by measuring the length of a set of waves and dividing by the number; though the measurement could not be made to a finer unit than 0.1^{mm} this reduces the error for a set of 5 waves to $\frac{1}{5}$ of 0.1^{mm}, or 0.02^{mm}. This method is applicable only when the vowel curve shows regular vibrations within a single period of the voice tone. When the curve shows irregular or complicated vibrations, some other method would be used.

HERMANN has used three other methods: (1) the centroid method, (2) the method of proportional measurement, and (3) the counting of the vibrations when they exactly fill one period of the voice tone.[1] The last method amounts to the same thing as mine for a particular case. The proportional method is also practically the same for other cases. The centroid method seems to give only approximate results.[2] The term "centroid" seems to me preferable to "center of gravity" used by HERMANN.

[1] HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 359.

[2] HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1893 LIII 51; 1894 LVIII 276.

Of all the methods and investigations employed for determining the mouth tone those of HERMANN[1] are entitled to by far the weightiest consideration. He finds for *u* (*oo*) two tones, one in the first part of the first octave and one in the second octave, for *o* (*au*), and *a* a tone in the second octave which rises in pitch as *o* changes to *a*, for *ä* and *e* a tone in the second octave and one in the third octave, for *ö*, *ü* and *i* a very high tone which is in the middle of the third octave for *ö*, at the end of that octave for *ü* and in the fourth octave for *i*. The octaves are numbered in the German fashion, middle *c* being in the first octave. The resonance tones for my examples of *a* and *i* are given on pages 55 and 56, and those of some other vowels in Section III.

These data give only the approximate regions in which we may expect to find the mouth tone. It is unquestionably true that within these regions the mouth tone will vary for different dialects and different conditions of speech.

The mouth tone need not be a fixed one though it is generally so. A rise and fall of the mouth tone might readily be used as a factor of expression in speech. Several examples of such changes have been given in Section II.

It seems fairly well established that in addition to the cord tone there may be several resonance tones from the mouth cavity. LLOYD distinguishes at least two: that of the front part of the mouth (the porch resonance) and that of the whole mouth (the fundamental resonance).[2] There may be also a resonance tone from the pharynx.[3] The various vowels arise from different "radical ratios" between the porch tone and the fundamental mouth tone,[4] while it is possible to change the pitch of both to some extent. Various other tones may arise from the configuration of the mouth and the coexistence of the tones already mentioned.[5]

Although LLOYD's supposition of the possible presence of a number of resonances in the mouth cavity[6] may be partly justified, yet one of these resonances must far exceed all others in prominence in order to produce the constancy in form and period of the resonance vibrations seen in the

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[1] HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LVIII 270.

[2] LLOYD, *Speech sounds; their nature and causation*, Phonet. Stud., 1890 III 261.

[3] LLOYD, *Speech sounds; their nature and causation*, Phonet. Studien, 1891 IV 294; also a note in Proc. Brit. Assoc., 1891 p. 796.

[4] LLOYD, *Speech sounds; their nature and causation*, Phonet. Stud., 1891 IV 52.

[5] Same, 207.

[6] LLOYD, *Speech sounds; their nature and causation*, Phonet. Stud., 1890 III 261; 1891 IV 52, 206.

curves examined in Section II. It is doubtful if there are more than two resonances of the mouth that are of any noticeable strength ; as explained above (p. 83) the air in a resonance cavity acts as a spring whose period depends on the size while the form of the cavity is immaterial for the chief resonance tone. We must add that, although the additional resonance tones and the overtones of the cord tone may not appear in any record, they undoubtedly give characteristic colors to the final result.

The importance of the pharyngeal resonance has been strongly emphasized by MARICHELLE.[1]

This author maintains the following theses : *A*. The capacity of the buccal resonator does not exercise a characteristic influence on the pitch of the vowels. The statement that the mouth cavity in front of the elevation of the tongue has no influence is based on an experiment in filling the cavity of the palate with wax and finding that the vowels *O* and *OU* can still be pronounced. Compensation for the size of the resonating cavity by change in the lip opening is avoided by forming the opening in a card placed before the mouth. These experiments seem to me too inaccurate and so contrary to our knowledge of the action of resonating cavities that we cannot accept them. Moreover, a vowel like *O* is—to my ear at least—distinctly modified in expression by any change of the mouth cavity although it still remains an *O* until the change is a great one. This can be conveniently tested by inserting two fingers in the mouth ; the *O* changes in expression and can be readily made into an *OU* by the proper manipulation. *B*. The dimension of the lip opening constitutes only a general vague and unstable indication of the vowel. *C*. The separation of the jaws does not sufficiently characterize the vocal sounds. *D*. The displacement of the tongue forward or backward furnishes no precise and essential information on the character [timbre] of the vowels. It is possible to produce all the vowels with practically any position of the tongue. "Here again the physiological description, as comprehended generally, gives only accessory facts and no characteristic ones." These three statements are true in a vague way but they do not prove that the vowel character is independent of these factors ; the vowels undoubtedly depend essentially and directly on them. MARICHELLE's point, however, seems to be that the essential factor is the size of the resonance cavity and not its exact form ; and in this he is presumably correct.

According to MARICHELLE three distinct regions of the mouth are used in forming vowels : 1. the anterior tongue-palate cavity ; 2. the pos-

[1] MARICHELLE, La parole d'après le tracé du phonographe, 27, Paris 1897.

terior tongue-plate cavity; 3. the lip opening. The characteristic tones are modified by *a.* the nature of the walls, whether soft or hard; *b.* the capacity of the posterior resonator; *c.* the degree of opening of the tongue-palate orifice; *d.* the lip opening.

MARICHELLE seems to be quite correct in insisting on the importance of the posterior cavity; it is the one into which the vibrations of the cords pass immediately and it undoubtedly acts as a strong resonator. It would be somewhat rash, however, to say that the most prominent resonance vibration comes from this cavity. It may be suggested that the vowel is a complex of resonance tones of which the pharyngeal tone would be one, the anterior mouth tone another, and so on.

The assumption of PIPPING[1] that the chief resonance tone of the vowels may be derived from the resonance of the chest seems to have little justification. The tone of the chest is a low one—my own has a frequency of about 100 complete vibrations a second—as can readily be determined by singing the scale; the chest resonance occurs only on very low notes. Its low pitch can also be heard by tapping the chest as in auscultation. The chest possibly resonates when very low tones are sung or spoken, but the pitch of ordinary speech is generally quite above it.

I believe we shall not go very far wrong if we assume that the entire mouth cavity may give rise to one resonance tone, the rear portion (pharyngeal) to another and the anterior portion to a third. Such an assumption has been made the basis of my attempt on p. 56 to explain the formation of *ai*.

VI. THE CORD TONE IN VOWELS.

Simple tones have three fundamental properties: pitch, intensity and duration. The so-called "timber" is not a property of simple tones, but the resulting effect of combinations of tones. In the present section it is proposed to discuss the cord tone in various vowels in regard to pitch and intensity. For this purpose only the fundamental tone of the vowel will be considered and no regard will be paid to the particular form of the curve resulting from the overtones of the cord tone and the superposition of the resonance tones. We will also assume that the vibration of the cords involves the usual supposition that the force of attraction to the position of equilibrium varies as the distance from that position. In such a case we can represent the fundamental tone by the equation

$$y = F(t) \sin \frac{2\pi t}{f(t)}$$

[1] PIPPING, *Zur Phonetik d. finnischen Sprache*, Mém. de la Société finno-ougrienne, XIV, Helsingfors 1899.

where $f(t)$ is the expression for the period of the vibration and $F(t)$ that for the amplitude. In this general expression the period and the amplitude may be constant or may vary with the time.

The pitch function.

A vowel during whose course the pitch remains constant can be said to be of "sustained" pitch. If T is the period of vibration of the cords, we have in the ideal case

$$y = F(t) \sin \frac{2\pi t}{T}.$$

Vowels of sustained or constant pitch are not very common in the cases I have studied. Most vowels seem to rise or fall, yet some of them are approximately constant. The vowel *i* as found in *see*, *needle*, *ai*, etc., is approximately a sustained vowel although it generally falls slightly. The following measurements of *i* in *see* are typical: 2.3, 2.3, 2.4, 2.4, 2.8^{σ} ... to the 22d vibration, 2.4^{σ} to the 42d vibration, 2.1^{σ} to the end at the 64th vibration.

The rather unusual case of two vowels of sustained pitch forming a diphthong is found in the word *my* of the phrase *With my bow and arrow*. The *a* has a constant period of 5.6^{σ} and the *i* that of 3.6^{σ}. The *a* has also a constant amplitude of 0.4^{mm}; the *i*, beginning with 0.5^{mm}, falls to 0 as usual in *ai* at the end of a word (see Section II.).

The diphthong *ai* is of nearly constant pitch throughout most of its length in the two cases of *thy* (Figs. 62, 67).

Nearly all vowels in the earlier parts of words in the record studied (p. 14), whether preceded by a consonant or not, are characterized by a rising pitch. In such a case the period is not a constant T but a function of the elapsed time, $f(t)$. A typical example of this kind of vowel is found in the *a* of *ai* (Section II.). A determination of the particular form of $f(t)$ for various vowels is a highly important matter, as different vowels and different manners of speaking are possibly characterized by different forms of this rise in pitch. Some of the cases of *a* suggest the form $f(t) = k\varepsilon^{-t}$, a formula which expresses many of the phenomena found in nature.

When the rise in pitch (decrease in period) is proportional to the elapsed time, we have

$$y = F(t) \sin \frac{2\pi t}{T_0 - mt}$$

where T_0 is the period of the first vibration and m the factor of proportionality. Such a vowel is found in the *a* of the 4th example of *I* above

(p. 28 and Fig. 29). During an interval of 180^σ its period is shortened by 5.5^σ, or at the rate of $0.03t$. Its cord equation on the suppositions made above would be (in seconds)

$$y = F(t) \sin \frac{2\pi t}{9 - 0.03t} .$$

In the latter portions of words the vowels in the records I have examined are generally nearly constant in pitch, with often a slight fall as the intensity decreases. Typical examples are found in the cases of *i* in *ai* (Section II.). This slight fall in pitch need not necessarily indicate a relaxation in the tension of the vocal cords; as the force of the expired current of air decreases, the frictional forces involved in the cord vibration may gradually lengthen the period. Yet the amount of fall is generally too great to be due to anything but a relaxation of the cords.

The amplitude function.

The intensity of a sound wave is to be defined as the amount of work performed by the passage of the wave through a unit surface in a unit time. It is directly proportional to the square of the amplitude and inversely proportional to the period. Complete calculations of the intensity of vowels under various circumstances may eventually be made; in the present investigation, however, the amplitude has been taken as the most convenient index of intensity.

In the records studied I have rarely found a vowel with a constant amplitude. Vowels at the beginnings of words show invariably a rise in amplitude. This rise may continue until the vowel ends in some other sound. Such is the case in *a* of *ai* (Section II.), and in *a* of *and* in *thread and needle*. Most vowels, however, rise to a maximum and then fall; as is typically illustrated in *a* (p. 67). Such vowels might possibly be called circumflex vowels. Even in the middle of the word the vowel has a tendency to the circumflex form, as is well shown in most cases of the *i* of *ai*. The rise and fall may be quite elaborate as in the case of the doubly circumflex vowel *o* of *bow*; this long *o*, however, might with propriety be considered a molecular union of two *o*'s in succession.

In a vowel of constant amplitude represented by the sinusoidal vibration we would have $F(t) = a$ and

$$y = a \sin \frac{2\pi t}{f(t)} .$$

In a rising vowel $F(t)$ might take some such form as mt, whence we would have $y = mt \sin \frac{2\pi t}{f(t)}$.

In a circumflex vowel we may assume the amplitude to be of sinusoid form whereby

$$F(t) = E \sin \frac{2\pi t}{s}$$

and

$$y = E \sin \frac{2\pi t}{s} \sin \frac{2\pi t}{f(t)}$$

where E would be the maximum amplitude and s the length of the vowel. When the pitch is constant the curve will have the form

$$y = E \sin \frac{2\pi t}{s} \sin \frac{2\pi t}{T}.$$

I have found one vowel, *a* in *said* in the line *I, said the sparrow*, that can be with close approximation considered as a circumflex vowel of constant pitch. Its equation is (in seconds and millimeters)

$$y = 0.5 \sin \frac{2\pi t}{0.108} \sin \frac{2\pi t}{0.0053}.$$

It does not fill a complete period of circumflexion as it is suddenly cut short by the *s* of *sparrow*.

Among the hundred or so English vowels that I have inspected, I have been unable to find one that can with any close approximation be considered as steady in intensity and constant in pitch. Thus a vowel of the form $y = a \sin \frac{2\pi t}{T}$ must be a rare one. Some vowels during part of their course are of this form, but a change of some kind seems characteristic at some moment. Even such approximations have been found only in the interior of words, that is, with boundaries of consonants or of vowels with the vocal organs already in action. It seems to be the rule in English that a vowel following a pause shall be a rising or crescendo one, and one preceding a pause shall be a falling or diminuendo one.

Sequence of cord tones.

There seems to be for a particular voice on a particular occasion certain tones around which the cord tones group themselves. BOEKE found that in ordinary speech his cord tone ranged from 181 to 256 complete vibrations.[1]

In the first stanza of *Cock Robin* the general tone seems to be one with a period of 5.3^σ (about 190 vibrations).

[1] BOEKE, *Mikroskopische Phonogrammstudien*, Arch. f. d. ges. Physiol. (Pflüger), 1891 L 297.

In addition to this a tone with a period of 7.0^{σ} (about 143 vibrations, making a musical interval of a fourth below the general tone) has a tendency to appear for the sonants of lower pitch and another tone with a period of 1.8^{σ} (about 560 vibrations, making a musical interval of a duodecime above the general tone) for the sonants of higher pitch.

The periods of the various sonants, as far as I have been able to determine them in this stanza, are given in thousandths of a second by the figures below them in the following quotation :

Wh o k i lled C o ck R o b i n
3.3 1.8 4.2 1.8 5.3 5.6 8.4

I, s ai d th e sp a rr ow,
18 to 4 5.3 5.3 5.3 2.8 5.2

W i th m y b ow a nd a rr ow
5.3 2.1 5.3 5.6–3.6 7.0 5.3 4.2 2.5 7.0

I k i lled C o ck R o b i n.
12 to 4 5.6 7.0 to 5.3 3.9 3.9 4.2 5.6 8.8

It may be suggested that the melodiousness of speech must depend to a great degree on the musical sequence of the cord and resonance tones.

VII. Verse-analysis of the 1st stanza of Cock Robin.

As stated on p. 1 these researches were begun in order to settle the controversy in regard to the quantitative character of English verse. A nursery rhyme was selected as being verse in the judgment of all classes of people for many ages. When compared with some of what many of us now consider to be the best verse, it shows various defects, but these defects are typical of the usual deviations from our present standards and are, moreover, not defects according to other standards. It is also a fact that our notions of verse are largely derived from the rhymes heard in childhood.

An analysis of the sounds of the first stanza is given in the four tables on the adjacent pages.

The first column gives the sounds in the phonetic transcription used by Vietor.[1] The second column gives the duration of each sound as determined by measurements of the curves in the records as described on p. 13. The third column gives the period of the cord tone, and the fourth gives the amplitude of the vibration in the tracing (p. 20), not the amplitude of the vibration on the gramophone plate or of the move-

[1] Vietor, Elemente der Phonetik, 3. Aufl., Leipzig 1894.

Line 1: *Who killed Cock Robin?*

Sound.	Duration in thousandths of a second.	Pitch (period in thousandths of a second).	Intensity (maximum amplitude in mm.).	Syllable effect.	Remarks.
h	10				Very short sound, not distinguishable in the record, not over 10σ in length. Compare with *h* on p. 60.
ū	180	3.3	0.4	strong	Forcible vowel, large amplitude in earlier portion, rises somewhat in pitch, average period 3.3. Compare with *ū* on p. 63.
k	119				Appears in the record as a straight line.
i	154	1.8	0.6	strong	Long vowel, large amplitude throughout, double circumflex in amplitude (p. 93). The high pitch of this *i* is in contrast with that of *killed* in the 4th line (below)
l	74	1.8	0.1		Compare p. 65.
d	0				No sound of *d* can be heard in this record; the record plate speaks "Who kill Cock Robin?"
k	53				Appears in the record as a straight line.
a	126	4.2	0.5	weak	Rises somewhat in pitch to 4.2 in the main portion, weak on account of lowness in pitch.
*	70				The vibrations of the *å* are suddenly cut short by a few vibrations of a different form that rapidly decrease in amplitude. In listening to the record plate the ear hears no glide between *å* and *h*; the word seems to be simply and distinctly *kåk* and not *kåak*. This glide seems to be, to the ear, an essential part of the *k*. The cords are still vibrating while the mouth is changing from the *å* position to the *k* position.
k	31				Straight line measured from * to *r*; there is no pause between *k* and *r*.
r	74	1.8	0.3		Very distinctly and heavily rolled *r*; pseudo-beats are apparent. Compare p. 69.
å	140	5.3	0.5	strong	Of very low but constant pitch; steady rise in intensity till the vowel is cut short by *b*; forcible on account of length and amplitude.
b	49				Straight line from *å* to *i*. Compare p. 67.
i	56	5.6	0.3	weak	Short but distinctly heard; weak on account of shortness, lowness and faintness.
n	74	8.4	0.2		Falls in pitch and amplitude.
•	770				

Line 2: *I, said the sparrow.*

Sound.	Duration in thousandths of a second.	Pitch (period in thousandths of a second).	Intensity (maximum amplitude in mm.).	Syllable effect.	REMARKS.
ai	452	18 to 4	0.7	strong	Full analysis on p. 16; strong by length, pitch of *i* and amplitude.
•	210				
s	?				Very brief sound, no trace in record.
e	105	5.3	0.5	weak	Rather long and loud, but low in pitch.
d	81	5.3	0.1		Pitch falls from 5.3.
dh	32	?	0.1		Very weak vibrations.
ə	84	5.3	0.2	weak	
sp	273				Impossible to distinguish between the two sounds in the tracing; the *s* is heard as a brief sound.
*	18	1.9	0.4		Distinct sound different from the following *æ*.
æ	170	5.3	0.5	strong	Constant very low pitch but steadily increasing amplitude; falls suddenly in intensity during 5σ to *r*; no sound of *ə* as stated in VIETOR, p. 115; strong on account of length and amplitude.
r	11	2.8	0.2		Clearly marked vibrations; the rolling of the *r* can be distinctly heard. Compare p. 69.
o	294	5.2	0.6	strong	Very long vowel of constant pitch, but of rising and then falling intensity (p. 93); strong by length and amplitude; followed without pause by *ū* of next line

ment of the cords. The fifth column gives what I consider to be the character of each syllable, whether strong or weak; the judgment is based on the sound of the gramophone record, aided by a study of the tables.

The elements in speech whose rhythmical arrangement is the essential of verse as contrasted with prose are: 1, quality; 2, duration or length; 3, pitch; and 4, intensity. The element of quality consists in the nature of the sound as a complex of tones and noises producing a definite effect as a speech-sound. Length, pitch and intensity are properties of the speech-sound that can be varied without destroying its specific nature, that is, without changing the quality. These four elements can be varied independently.

It seems to be sufficiently well settled that, in addition to variations of quality, that is, of the speech-sounds, the essential change in Greek verse was one of pitch. I have observed a similar characteristic in Japanese

Line 3: *With my bow and arrow.*

Sound.	Duration in thousandths of a second.	Pitch (period in thousandths of a second).	Intensity (maximum amplitude in mm).	Syllable effect.	Remarks.
ŭ	108	5.3	0.2		Amplitude rises from 0.
i	60	2.1	0.4	strong	Circumflex sustained vowel; compare p. 94; strong by pitch and amplitude.
dh	50	?	0.1		
m	74	5.3	0.1		
a	170	5.0	0.4	strong	Both parts of this diphthong are nearly constant in pitch and amplitude; compare p. 92; strong by length and amplitude.
i	112	3.0	0.5		
◡	140				*My* is followed by a brief rest in order to bring out the *b* distinctly. The *b* makes no curves in the record.
b					
o	490	7.0	0.4	strong	Extremely long vowel of very low pitch with two maxima of intensity; it might be considered as a close succession of two *o*'s; compare p. 93; strong by length and amplitude.
◡	11				
æ	382	7.7–5.3	0.2	weak	The *æ* begins at a very low pitch 7.7 and rises steadily to 5.3, which is maintained throughout the *n*. The form of the curve for *æ* differs from that for *n*, yet the change is so gradual that it is impossible to assign any dividing line.
n		5.3	0.1		
d	18				Straight line in the record.
*	102	5.3	0.4		This extra vowel arises from the attempt at extra distinctness in speaking.
æ	189	4.2	0.3	strong	Strong by length and pitch.
r	39	2.5 (?)	0.1		Rolled *r*, brief.
o	331	7.0	0.6	strong	A single vowel of circumflex intensity; compare p. 93; strong by length and amplitude.
◡	420				

verse. Probably no better way of getting an idea of the nature of Greek verse could be found than that of listening to typical Japanese verse. I have also found another form of pitch-verse in a kind of poetical dictionary used by the Turks for learning Persian.

Latin verse was essentially a time-verse, the chief distinction among the syllables being that of length in addition to the change in speech-sounds.

English verse is usually considered to be an intensity-verse, or a verse of loud and soft syllables. The four tables show quite evidently that English verse is also a pitch-verse and a time-verse.

It may be said that in all probability changes of length and intensity

Line 4: *I killed Cock Robin.*

Sound.	Duration in thousandths of a second.	Pitch (period in thousandths of a second).	Intensity (maximum amplitude in mm.).	Syllable effect.	Remarks.
ai	334	12–4	0.6	strong	Full analysis on p. 22; strong by length, pitch of *i* and amplitude.
k	125				Straight line in the record.
i } *l* }	324	5.6	0.2	weak	It is impossible to assign any definite point as the limit between these two sounds; weak, low *i* in contrast to the *i* in the first line above.
d	33				This *d* is distinctly heard; compare *d* in first line above.
*	81	4.9	0.2		Additional vowel due to the extra distinctness in speaking the *d;* it arises from the explosive opening of the mouth; the pronunciation of the word *killed* is different from that in the first line chiefly in the great difference in pitch and in the greater distinctness of the *d*.
k	133				Straight line in the record
a	147	7.0–5.3	0.3	weak	Pitch rises from beginning to end.
÷	76				See the same word in the first line above.
k	46				Straight line in the record.
r	60	3.9	0.6 }	strong	The *r* is more vowel-like than the corresponding *r* in the first line; the strong roll is not heard; the curve of *ro* very much resembles in period and amplitude the curve of an *ai* in *thy* (Fig. 61) turned backward; the period of the cord tone is practically constant; the resonance tone of the mouth undergoes a continuous change; any assignment of a limit between the two sounds must be somewhat arbitrary; the sound *ro* is strong by length, pitch and amplitude.
a	103	3.9	0.5 }		
b	53	4.2	0.1		The *b* cuts off suddenly the sound of *o*.
i	82	5.6	0.4 }	weak	The *i* is heard, but not so distinctly as in the first line above.
n	74	8.8	0.1 }		Weak, low, diminuendo.
⌣	955				

went along with the changes of pitch in Greek verse but that they were of minor importance. Perhaps, also, changes of pitch and intensity likewise accompanied the long and short syllables in Latin verse. But I do not think that for English verse we can fully accept the analogous statement that, although the changes in pitch and length may be present,

they are quite subordinate to the changes in intensity. It would, I believe, be more nearly correct to say that English verse is composed of strong and weak, or emphatic and unemphatic syllables and that strength can be produced by length, pitch or intensity.

The usual scansion of this stanza in strong and weak syllables would give

— — ⏑ — ⏑
— ⏑ ⏑ — —
— — — ⏑ — —
— ⏑ ⏑ — ⏑

The three elements: length, pitch and intensity, are all used to produce strength. Thus the forcible vowel $\bar{u}$ in Line 1 is long and moderately high and loud.

The strength of a syllable may be kept the same by increasing one of the factors as another one decreases. The vowel *o* of *Robin* in Line 1 is strong on account of its length and intensity, although its pitch is low. A syllable necessarily short may be made as strong as a longer one by making it louder or higher; or a syllable necessarily of small intensity may be strengthened by lengthening it or raising its pitch. Thus, the short *i* of *With* in Line 3 is strong on account of its high pitch and large amplitude; and the weak *æ* of *arrow* in Line 3 is strong on account of its high pitch and its length. This might be called the *principle of substitution.*

An increase in the loudness, length or pitch of a syllable renders it stronger—other things being equal. Using the symbol f to indicate dependence we may put $m = f(x, y, z)$, where m is the measure of strength and x, y and z are the measures of intensity, length and pitch respectively. This might be called the fundamental *principle of strength.*

The study of this and other specimens of verse has made it quite clear that the usual concept of the nature of a poetical foot is erroneous in at least one respect. *Lines* in verse are generally distinct units, separated by pauses and having definite limits. A single line, however, is not made up of smaller units that can be marked off from each other. It would be quite erroneous to divide the first stanza of Cock Robin into feet as follow.

Who killed|Cock Rob|in?
I, said the|sparrow.
With my bow|and ar|row
I killed|Cock Rob|in.

No such divisions occur in the actually spoken sounds and no dividing points can be assigned in the tracing.

The correct concept of the English poetical line seems to be that of a certain quantity of speech-sound distributed so as to produce an effect equivalent to that of a certain number of points of emphasis at definite intervals. The proper scansion of the above stanza would be:

Who killed Cock Robin?
I, said the sparrow,
With my bow and arrow
I killed Cock Robin.

The location of a point of emphasis is determined by the strength of the neighboring sounds. It is like the centroid of a system of forces or the center of gravity of a body in being the point at which we can consider all the forces to be concentrated and yet have the same effect. The point of emphasis may lie even in some weak sound or in a mute consonant if the distribution of the neighboring sounds produces an effect equivalent to a strong sound occurring at that point. Thus the first point of emphasis in the third line lies somewhere in the group of sounds *mybow*, probably between *y* and *o*.

With this view of the nature of English verse all the stanzas of Cock Robin can be readily and naturally scanned as composed of two-beat or two-point lines.

It is not denied that much English verse shows the influence of quantitative classical models, but such an influence is evidently not present in Cock Robin.

Thanks are due to Prof. Hanns Oertel who has very kindly read most of the proof of this article; he has enriched it by various suggestions particularly in regard to the *h* discussed on p. 60.

OBSERVATIONS ON RHYTHMIC ACTION[1]

BY

E. W. SCRIPTURE.

Two entirely different forms of regularly repeated action are to be distinguished. In one form the subject is left free to repeat the movement at any interval he may choose. This includes such activities as walking, running, rowing, beating time, and so on. A typical experiment is performed by taking the lever of a MAREY tambour between thumb and index finger and moving the arm repeatedly up and down; the recording tambour writes on the drum the curve of movement. Another experiment consists in having the subject tap on a telegraph key or on a noiseless key and recording the time on the drum by sparks or markers. Other experiments may be made with an orchestra leader's baton having a contact at the extreme end, with a heel contact on a shoe, with dumb-bells in an electric circuit, and so on. For this form of action I have been able to devise no better name than "free rhythmic action."

In contrast with this there is what may be called "regulated rhythmic action." This is found in such activities as marching in time to drum-beats, dancing to music, playing in time to a metronome, and so on. A typical experiment is that of tapping on a key in time to a sounder-click, the movement of the finger being registered on a drum.

Regulated rhythmic action differs from free rhythmic action mainly in a judgment on the part of the subject concerning the coincidence of his movements with the sound heard (or light seen, etc.). This statement, if true, at once brushes aside all physiological theories of regulated rhythmic action. One of these theories is based on the assumption (EWALD) that the labyrinth of the ear contains the tonus-organ for the muscles of the body. It asserts that vibrations arriving in the internal ear affect the whole contents, including the organ for the perception of sound and the tonus-organ. Thus, sudden sounds like drum-beats or emphasized notes would stimulate the tonus-organ in unison, whereby corresponding impulses would be sent to the muscles. This theory has very much in its favor. It is undoubtedly true that such impulses are sent to the muscles. Thus at every loud stroke of a pencil on the desk I can feel a resulting

[1] Reprinted from Science, 1899 X 807.

contraction in the ear which I am inclined to attribute to the *M. tensor tympani*. Likewise a series of drum-beats or the emphasized tones in martial or dance music seem to produce twitching in the legs. FÉRÉ has observed that, in the case of a hysterical person exerting the maximum pressure on a dynamometer, the strokes of a gong are regularly followed by suddenly increased exertions. Nevertheless, these twitchings are not the origin of the movements in regulated rhythmic action. For many years I have observed that most persons regularly beat time just before the signal occurs; that is, the act is executed before the sound is produced. Records of such persons have been published,[1] but their application to the invalidation of the tonus-theory was first suggested by Mr. Ishiro MIYAKE. This does not exclude the use of muscle sensations, derived from tonus-twitches, in correcting movements in regulated rhythmic action, although they presumably play a small or negligible part as compared with sounds.

Another argument in favor of the subjective nature of regulated rhythmic action is found in the beginning of each experiment on a rhythm of of a new period; the subject is quite at loss for a few beats and can tap only spasmodically until he obtains a subjective judgment of the period. If the tonus-theory were correct, he should tap just as regularly at the start as afterward.

The conclusion seems justified that regulated rhythmic action is a modified free rhythmic action, whereby the subject repeats an act at what he considers regular intervals, and constantly changes these intervals to coincide with objective sounds which he accepts as objectively regular.

In free rhythmic action there is one interval which on a given occasion is easiest of execution by the subject. This interval is continually changing with practice, fatigue, time of day, general health, external conditions of resistance, and so on.

"It has long been known that in such rhythmic movements as walking, running etc., a certain frequency in the repetition of the movement is most favorable to the accomplishment of the most work. Thus, to go the greatest distance in steady traveling day by day the horse or the bicyclist must move his limbs with a certain frequency; not too fast, otherwise fatigue cuts short the journey, and not too slow, otherwise the journey is made unnecessarily short. This frequency is a particular one for each individual and for each condition in which he is found. Any deviation from this particular frequency diminishes the final result."

It is also a well-known fact that one rate of work in nearly every line is peculiar to each person for each occasion, and that each person has

[1] SCRIPTURE, New Psychology, 182, London 1897.

his peculiar range within which he varies. Too short or too long a period between movements is more tiring than the natural one in walking, running, rowing, bicycling, and so on.

It is highly desirable to get some definite measurement of the difficulty of a free rhythmical action. This cannot well be done by any of the methods applicable to the force or quickness of the act, but it may be accomplished in the following manner.

As a measure of the irregularity in a voluntary act we may use the probable error. When a series of measurable acts are performed they will differ from one another, if the unit of measurement is fine enough. Thus, let x_1, x_2, $\cdots$, x_n be successive intervals of time marked off by a subject beating time, or walking, or running, at the rate he instinctively takes. The average of the measurements,

$$a = \frac{x_1 + x_2 + \cdots + x_n}{n},$$

can be considered to give the period of natural rhythm under the circumstances. The amount of irregularity in the measurements is to be computed according to the well-known formula:

$$p = \sqrt{\frac{v_1^2 + v_1^2 + \cdots + v_n}{n - 1}}$$

where $v_1 = x_1 - a$, $v_2 = x_2 - a$, $\cdots$, $v_n = x_n - a$. The quantity p is known as the "probable error," or the "probable deviation." The quantity

$$r = \frac{p}{a},$$

the "relative probable error," expresses the probable error as a fraction of the average.

If all errors in the apparatus and the external surroundings have been made negligible, this "probable error" is a personal quantity, a characteristic of the irregularity of the subject in action. If, as may be readily done, the fluctuations in the action of the limbs of the subject be reduced to a negligible amount, this probable error becomes a central, or subjective, or psychological, quantity. Strange as it may appear, psychologists have never understood the nature and the possibilities of the probable error (or of the related quantities "average deviation," "mean error," etc.). In psychological measurements it is—when external sources of fluctuation are rendered negligible—an expression for the irregularity of the subject's mental processes. Nervous or excitable

people invariably have large relative probable errors; phlegmatic people have small ones.

Thus a person with a probable error of 25% in simple reaction time will invariably have a large error in tapping on a telegraph key, in squeezing a dynamometer, and so on. I have repeatedly verified this in groups of students passing through a series of exercises in psychological measurements. I do not believe it going too far to use the probable error as a *measure* of a person's irregularity. This is equivalent to asserting that a person with a probable error twice as large as another's is twice as irregular, or that if a person's probable error in beating time at one interval is r_1 and at another interval r_2, his irregularity is r_1 times as great in the second case as in the first. This concept is analogous to that of precision in measurements. We might use the reciprocal of the probable error as a measure of regularity. The positive concept, however, is in most minds the deviation, variation or irregularity, and not the lack of deviation, the non-variability, or the regularity. In the case of the word "irregularity" the negative word is applied to a concept that is naturally positive in the average mind.

The irregularity in an act is a good expression of its difficulty. Thus, if a person beating time at the interval T has an irregularity measured by the probable error P and at the interval t a probable error p it seems justifiable to say that the interval t is $\frac{p}{P}$ times as difficult as T. If T is the natural interval selected by the subject, then the artificial interval t would be more difficult than T, and we should measure the difficulty by comparing probable errors.

It is now possible to state with some definiteness the law of difficulty for free rhythmic action. Let T be the natural period and let its probable error—that is, its difficulty—be P. It has already been observed (Science, 1896, N. S. IV 535), that any other larger or smaller period (slower or faster beating) will be more difficult than the natural one and will have a larger probable error. Thus any interval t will have a probable error p which is greater than P, regardless of whether t is larger or smaller than T.

Three years ago (Science, as above) I promised a complete expression for this law. Continued observations during this time enable me to give an idea of its general form. The results observed can be fairly well expressed by the law

$$p = P\left(1 + c\,\frac{[t - T]^2}{t}\right)$$

in which T is the natural period, P the probable error for T, t any arbitrary period, p the probable error for t and c a personal constant.

This may be called the law of difficulty in free rhythmic action. A curve expressing the equation for $T = 1.0^s$, $P = 0.02^s$ and $c = 1$ is given in the figure.

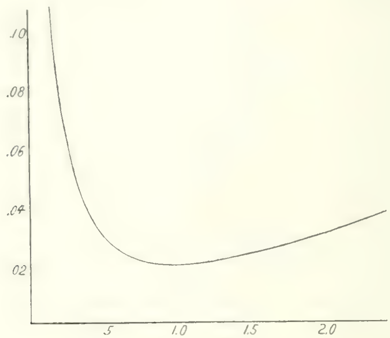

FIG. 75.

It will be noticed that periods differing but little from the natural one are not much more difficult and that the difficulty increases more rapidly for smaller than for larger periods.

In plotting this curve I have assumed unity as the value for all personal constants. The personal constants will undoubtedly vary for different persons, for different occasions and for different forms of action; an investigation is now in progress with the object of determining some of them.

In case it is desired to know what periods are of a difficulty 2, 3, ···, n times that of T, a table of values for p may be drawn up in the usual way and that value for t sought (with interpolation) which gives for p a value 2, 3, ···, n times as great. Thus, in a table for the above example it is found that the periods 0.38^s and 2.6^s are twice as difficult.

This law can be stated in another form which is of special interest to the psychologist. To the person beating-time a period of 0 is just as far removed from his natural period as one of ∞; both are infinitely impossible. The objective scale does not express this fact; objectively a period of 0 is as different from a period of 1^s as a period of 2^s would be. Similar considerations hold good for the lesser periods; the scale by which the mind estimates periods is different from their objective scale. This difference may be expressed by asserting that the following relations exist between the two:

$$x = c\,\frac{(t - T)^2}{t}$$

where x is the measure on the mental scale, T the natural period, t any other period, and c a personal constant. By this formula the various periods may be laid off according to their mental differences from the natural period. Every difference from the natural period is mentally a positive matter. With the mental scale the law of difficulty becomes

$$p = P(1 + cx)$$

where p and P are the probable errors for t and T respectively, x is the measure on the mental scale and c is a personal constant. This is the equation of a straight line. The law states that the difficulty of any arbitrary period is directly proportional to its mental difference from the natural period. This is the statement which I tried to make in the note published in Science, 1896, N. S. IV 535.

This law of difficulty as depending on the period is, of course, only one of the laws of free rhythmic action. It is quite desirable that other laws of difficulty and of frequency should be determined. For example, observations on ergograph experiments tend to show that the irregularity and the natural period both change with the weight moved; they also change with the extent of the movement.

Such a series of well established laws might be useful in regulating various activities to the best advantage. It is already recognized that it is most profitable to allow soldiers on the march to step in their natural periods; it is also known that on the contrary sudden and tense exertion is favored by changing the free rhythmic action into regulated action by marching in step and to music. More definite knowledge might perhaps be gained concerning the most profitable adjustments of the rhythm and extent of movement in bicycle-riding to the person's natural period; at present only average relations are followed in the adjustment of crank-length, gear and weight to bicycle-riders, individual and sex differences not being fully compensated. Other examples will suggest themselves.

Not only does every simple activity have its own natural rhythms; combinations of activities have rhythms that are derived from the simpler ones. In fact, it may be said that the individual, as a totality, is subjected to a series of large rhythms for his general activity (e. g., yearly, monthly, weekly, daily, and so on), and also to a series of smaller rhythms for his special activities. The natural periods do not always correspond with the enforced periods. The daily rhythm is unquestionably too slow for some persons and too rapid for others; the unavoid-

able enforcement of the 24-hour period works a loss to all who would naturally vary from it, and diminishes the total amount of work that could be produced by them. For large numbers of brain-workers the 24-hour period is too long; for many of them the natural period is probably about 18 hours. Although about one-quarter of the day is not efficiently used, there is little relief in splitting up the day into parts, because (1) the 12-hour period would be naturally even less advantageous than the 24-hour one, and (2) the new rhythm cannot be made to fit the environment.

The progress of civilization and the changes in life are undoubtedly tending to shorten the natural period from 24 hours by encouraging a greater discharge of energy at shorter intervals. Since the 24-hour rhythm is a fixed one, there must be a constant effort at adjustment in this respect by those individuals most susceptible to the new influences. the survival of the fittest will, of course, tend to keep the natural rhythm not far from the 24-hour period.

STUDIES

FROM THE

Yale Psychological Laboratory

EDITED BY

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1900

VOL. VIII.

YALE UNIVERSITY
NEW HAVEN, CONN.

PRESS OF
THE NEW ERA PRINTING COMPANY,
LANCASTER, PA.

CONTENTS

A TEST OF SAFE COLOR VISION

BY

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The problem that confronts the examiner of the color vision of applicants for positions on the railway and on shipboard has been frequently misunderstood. The usual test with colored wools aimed at picking out two classes of men with so-called green-blind and red-blind vision. The object of any test for color vision employed in these two services should be to pick out the men who can with absolute safety be trusted to recognize the signal colors. The two systems of testing are quite opposite: the one picks out and excludes men conspicuously unsafe and lets all the others into the service, while the other picks out and passes only the absolutely safe ones and excludes all others. When human life may be endangered by a mistake, the only admissible test is the latter one. It is my opinion that one, and only one, test of color vision should be used by examiners: namely, one that proves every person tested by it to be of safe or of unsafe color vision.

Principles for a test of color vision.

The fundamental requirement for a test of color vision is that of *reliability;* the test must with absolute certainty exclude all forms of color defect that would interfere with service on the railway and on shipboard, and must not exclude persons who do not possess such defects.

This requirement is not met by the wool test. In the first place there are many cases of rejection for color blindness which later examination proves to have been unjustified. There are also many cases of color blindness discovered after the person has passed the test; these are regularly treated as cases of "acquired" color blindness without the certainty that they are not sometimes cases of overlooked color blindness.

On the other hand, railway and marine surgeons who use a test with colored glasses in addition to the wool test, constantly report the discovery of cases by the one form of test which have successfully passed the other one. This would indicate that neither the wool test nor the usual test with colored glasses is reliable.

The first principle to be adopted for a test is that of the closest possible *resemblance to the actual conditions* under which lights are to be

judged. In practice a railway man or a pilot is required to decide for himself which of the lights he sees are red, green and white. An ideal test would consist of an immense number of such lights under all the variations of brightness, distance, fog, etc. Any concrete test should consist in the reduction of this ideal to a convenient form.

The usual wool test fails in respect to this principle. In practice the railway man or pilot is never required to put into a pile all the objects that resemble a certain sample.

The second principle is derived directly from the first one: the objects used in the test should *resemble the objects* in actual practice. Colored lights would conform to this principle.

A third principle may likewise be deduced from the first one: the person tested should be required to *name the objects.* This is just what he does in thinking about the lights he sees when at work; he decides that this one is "red," that one is "green," etc.

The judgment of the likeness or unlikeness of colored objects is an utterly different matter and involves a different form of mental activity. Great trouble and uncertainty arise in the wool test because it demands a different form of judgment from that to which the men have been accustomed.

The usual objection to having the colors named arises from the fact that such naming is inadmissible in the Holmgren test.

A fourth principle may also be deduced from the first one: several colors should be *presented simultaneously* for comparison. In practice the person sees several lights together; he compares them and decides on their colors. The secret of the past success of the Holmgren test over the tests of the Donders form with colored glasses lies in its use of the principle of comparison. The ideal test would combine this principle with the other three in which the Holmgren test fails.

The color sense tester as first devised[1] has undergone several changes. The principles on which it is based were first published[2] in outline in 1899; the foregoing account summarizes them briefly. The present account of the first model is reproduced with some changes from an account in the *Proceedings of the IV. International Congress of Psychology*, Paris, 1900. The second model is here described for the first time.

The color sense tester (first model).

In general appearance the color sense tester resembles an ophthalmo-

[1] Scripture, *[illegible]*. Stud. Yale Psych. Lab., 1895 III 103.

[2] Scripture, *[illegible]*. Proceedings of the N. Y. Railway Club, No. [illegible], [illegible]

scope. On the side toward the person tested, Fig. 1, there are three windows of glass 8^{mm} in diameter, numbered 1, 2 and 3 respectively. The opposite side of the tester, Fig. 2, consists of a movable disk carrying twelve glasses of different colors. As this disk is turned by the finger of the operator the various colors appear behind the three windows.

FIG. 1.

FIG. 2.

At each movement of the disk the subject calls off the colors seen at the windows. Number 1 carries a very dark smoked glass; all colors seen through it will be dark. Number 2 has no glass, showing all colors in full brightness. Number 3 carries a light smoked glass. There are thus thirty-six possible combinations of the colors. The twelve glasses are, however, reds, greens and grays.

A suitable arrangement of the colors gives direct simultaneous comparisons of reds, greens and grays of different shades. The well-known

confusion by color-defectives of dark greens with reds, greens with gray, etc., are exactly imitated, and the instrument gives a decisive test for color blindness. A peculiar advantage, however, lies in the fact that it presents reds, greens and grays simultaneously in a large number of different shades of intensity. The light of a green lantern, as it appears to a color defective at different distances, is simulated by the red behind the darker gray; at the same time a white light is also changed. The color defective to whom weak green is the same as gray (white at a distance) is confused and thinks that the weakened green is gray (white) and the dark gray is green.

The actual test is performed in the following manner. The tester is held before a window or a white surface, but not in the bright sunlight, at about 5 meters from the person tested. The operator begins with any chance position of the glasses, and asks the person tested to tell the colors seen through the three glasses, numbers 1, 2 and 3. He answers, for example, "number 1 is dark red; number 2 is gray; number 3 is green." The operator records from the back of the tester the letters indicating what glasses were actually used. Suppose he finds that A, D and G were opposite the glasses numbered 1, 2 and 3, he records: A_1, dark red; D_2, gray; G_3, green. The disk is then turned to some other position, the colors are again named, and the operator records the names used. For example, the result might be: "number 1 is dark green; number 2 is white; number 3 is red"; and the record would read: G_1, dark green; J_2, white; A_3, red. Still another record might give: J_1, dark gray; A_2, red; D_3, medium gray. Similar records are made for all combinations. Of course, the person tested knows nothing concerning the records made.

The records can be taken by anyone, and, on the supposition that the record has been honestly obtained and that the instrument has not been tampered with after leaving the central office, the comparison is likewise mechanical. There is none of the skillful manipulation required in the wool test and none of the uncertainty attaching to its results. The only instruction given to the subject is "Name the colors"; the results render the decision with mechanical certainty.

The requirements of simplicity and portability have, I hope, been successfully met; at the same time the instrument is thoroughly scientific in its principles.

Three colors at a time make a number agreeable to the subject; more would probably distract him. It is a fact well established by experimental psychology that under ordinary circumstances the mind cannot well attend to more than five objects at a time; the number 3 is within the limit.

The kind of comparison required is exactly the kind demanded in practice. The pilot, for example, is never required to decide if one color is "like" another, but to decide which colors are red, which green and which white. The HOLMGREN test requiring a judgment of "likeness" or "unlikeness" forces the subject to go through a difficult and complicated mental process. With the color sense tester the subject, seeing three colors at a time, passes judgment as to what colors they are.

The slide shown in Fig. 3 renders it possible to carry out tests quantitatively after the method proposed by DONDERS. The slide contains holes with the diameters of 1, 2, 3, 4, 5, 6, 7, 8 ^{mm} respectively. Using a standard source of illumination and placing the person tested at a given distance, the examiner can determine the smallest opening with which a certain color, say red, can be distinguished. If the smallest opening as determined for the average person be R^{mm} and for the person tested r^{mm}, the sensitiveness can be said to be indicated by $S = \frac{r}{R}$. The other quantitative test can likewise be readily carried out; a standard opening, say 1^{mm} is selected and the person tested is made to approach the instrument until he can distinguish the color. If D^{mm} is the average distance found and d^{mm} that for the person tested, his sensitiveness can be indicated by $E = \frac{d}{D}$.

Cases of color defect of the central part of the eye only are generally due to the excessive use of tobacco, especially when combined with the abuse of alcohol. To detect these cases the slide (Fig. 3) is placed in

FIG. 3.

window number 2 of the tester, and the person is required to call off the colors seen through the opening of 1^{mm} or 2^{mm} while the examiner shows them in rapid succession. The rapid succession is needed to allow no time for the person to turn his eye sidewise so that the colors fall outside the center.

The tester can be carried readily in the pocket, and can be used wherever a light of medium intensity is available.

When portability is not desired, the tester can be used in front of a properly arranged lantern. The ordinary semaphore lantern may be readily adapted to the purpose as shown in Fig. 4 by putting a cap and a small support in front of the lens. I use a semaphore lantern contain-

ing a sixteen candle power incandescent lamp and a sheet of white glass. The tester is simply laid in the small projecting arm in front of the glass. The examiner can see the letters marked on the edge of the tester. This form of the test is highly convenient for a permanent station, as it is always ready with the correct light.

These testers are now in use in several places. One examiner writes that "the men examined say that this test is more like the signals they

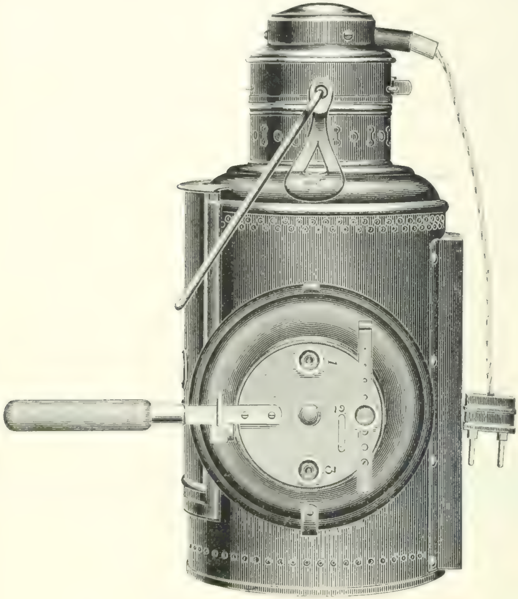

FIG. 4.

are used to seeing every day on the road, and is therefore fairer than to ask them to pick out a lot of delicately tinted pieces of yarn." Yet this same examiner relates that he has by means of the tester caught a number of men who slip through the wool test.

There remains the question: Is this test alone sufficient for full protection of the traveling public? A proper test should be absolutely reliable. The employment of several tests indicates that none of them is reliable. I believe that the color sense tester is thoroughly scientific in its

character and that it is reliable and decisive. Moreover, I have in my experience caught with it a number of men who had passed the wool test, and have never found one who passed the tester and yet showed any indication of color blindness in any way.

Instructions.

The following instructions are issued for the use of the examiner:

The eyes should be examined separately; a small blinder may, if desired, be hung over the eye not in use.

Hold the tester toward a bright surface (a window, the sky or the glass of a lantern) directly in front of the person to be examined, at a distance of five meters from his eye. The side with the three windows should be toward him. Ask him to call off the colors of these three windows in their order, beginning at the left window marked 1: thus, he is to say "Number 1 is such or such a color; number 2 is such or such a color; number 3 is such or such a color." You, the examiner, are then to notice the letters under the colored circles behind the three windows and to note the results on the record blank in the proper spaces. Thus, if he answers "Number 1 is red; number 2 is gray; number 3 is green," you look at the back of the tester and find, for example, that glass G is opposite number 1, D opposite number 2 and A opposite number 3. On the record blank you find a line containing in three blank spaces numbered G_1, D_2, A_3 and you write the answers in these places. While making the record you will have laid the tester down. You now turn the movable disc one or two or more notches either way; you hold the tester before the window and ask for answers as before. In this way you proceed until the record blank has been completely filled. A comparison of the answers with a list of the actual colors will at once reveal the defect if present. When color blindness is present, it is well to write the true colors in red ink in the same space with the answers of the person examined.

Occasionally it occurs that the person examined has a central defect of the field of vision which affects his perception of the object directly looked at if it is small. To detect this defect, place under the central window of the tester, the slide having the small holes. Place the 1^{mm} or 2^{mm} hole in the middle of the window. Ask him to call off the colors as you turn the movable disc with considerable rapidity. The rapidity is advised in order to hinder the person from forming an opinion by using a side portion of the eye.

At the bottom of the blank the decision should be recorded as to the good or defective color sense of the person examined. No attempt

should be made to state whether an existing defect is due to "red blindness" or "green blindness" or "color weakness," as such theoretical matters are still unsettled, the most common forms of color blindness being probably neither red nor green blindness. The question for you to decide is: Can this person distinguish and recognize with absolute certainty the lights used in practical service? The foregoing test will give the answer and any further testing or experimenting is a personal matter with the examiner.

Record blank.

Examination with E. W. Scripture's Color Sense Tester No.

Place .... Date

Name of Examiner

Name of Person examined

Age .... Residence

Remarks

Left eye.			Right eye.		
A_1	J_2	G_3	A_1	J_2	G_3
B_1	K_2	H_3	B_1	K_2	H_3
C_1	L_2	I_3	C_1	L_2	I_3
D_1	A_2	J_3	D_1	A_2	J_3
E_1	B_2	K_3	E_1	B_2	K_3
F_1	C_2	L_3	F_1	C_2	L_3
G_1	D_2	A_3	G_1	D_2	A_3
H_1	E_2	B_3	H_1	E_2	B_3
I_1	F_2	C_3	I_1	F_2	C_3
J_1	G_2	D_3	J_1	G_2	D_3
K_1	H_2	E_3	K_1	H_2	E_3
L_1	I_2	F_3	L_1	I_2	F_3

The examiner must answer the following questions by "Yes" or "No."

1. Do the results of the tests with large openings indicate *safe* color vision for the left eye?.........for the right eye?.........

2. Does the test with the small opening indicate a central defect for the left eye?.........for the right eye? ....

THE COLOR SENSE TESTER (SECOND MODEL).

The second model of the color sense tester differs from the first in providing for a much greater number of combinations of colors. The handle supports a brass frame holding a piece of white glass in front of which are ten circular holes. This provides ten windows of evenly dis-

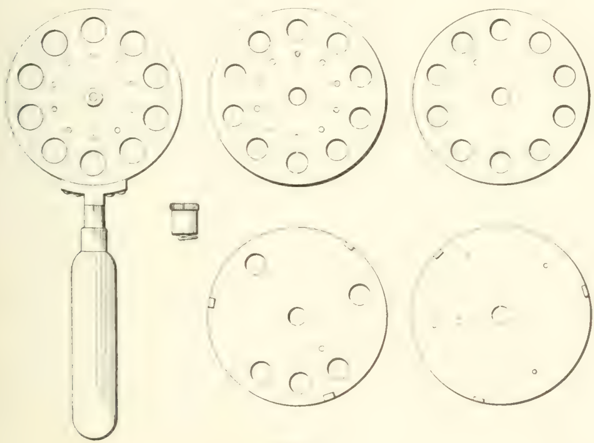

FIG. 5

tributed white light. In addition to this part there is first a disc with ten windows of glass of the colors used for signaling. These colors are carefully matched with the signal colors and tested by the spectroscope; the disc is lettered along its edge. Another disc of similar arrangement carries ten shades of glass from clear glass to the darkest smoke-glass that can be obtained. A number is placed on the edge opposite each glass. Two diaphragms are also provided. One has five holes of the full size of the windows so arranged that as the diaphragm is turned in front of the windows each window is at some time compared with every other window. The other diaphagm has five holes of 1^{mm} diameter arranged in the same way.

The lantern for this tester may be used with either a small incandescent electric lamp or an oil light. For permanent testing stations a semaphore lantern with an 8 c.p. incandescent lamp of the candle shape has been found convenient. The back window of the lamp serves for recording results when the room is darkened. In darkness the letters and numbers on the edges of the discs are read by lifting the tester from its support; it can be lifted or replaced by a single movement of the hand. Usually it will be found sufficient to note only the mistakes of the person tested, recording the name given by him and the true name of the color. Practice and experience will soon enable the examiner to conduct the test efficiently with no other record than the final "safe" or "unsafe."

Instructions.

The instructions for making the test differ somewhat from those with the first model.

The examination should be made in a dark room. The eyes should be tested separately; a small blinder may be hung over the eye not in use.

Place the lantern on a table at the height of the eyes of the person tested and at ten feet (three meters) from him. Place the tester in the catches on the front of the lantern.

Three separate tests are to be made.

First test.

Use only the disc with the colored glasses and the diaphragm with five large holes.

Tell the person to call off promptly the colors he sees, beginning with the color farthest from all the others or with any one you may indicate by covering it a moment with the finger and proceeding around in the direction of the hands of a clock.

Note down any mistakes that are made. Thus, if he calls yellow "red," you should note down under the heading of Test I: Yellow called red. Confusions between yellow and white are not to be considered as mistakes, because the source of light is itself yellow, although ordinarily called white.

Turn both disc and diaphragm irregularly and again call for the names. Note mistakes as before.

Repeat about 10 times.

A person who makes mistakes in this test can be at once declared of unsafe color vision without further testing.

In testing it is well to use as few words as possible. Thus, the instruction should be given: "Name the colors in this order," etc. It is also

proper to demand, "What is this color that I cover with my finger for a moment," but it is not proper to ask, "Is this color red?" and the like.

Second test.

Use only the disc with the colors and the diaphragm with 5 small holes.

The test is conducted in the same way as the first test, except in requiring the answers with great rapidity.

The object of this test is to detect cases of color defect that are confined to the central spot of the eye. The naming must be done with sufficient rapidity to prevent the person from using the side portions of the eye.

Third test.

Place the disc with colors first on the axle, then on that the disc with smoke-glasses and finally the diaphragm with 5 large holes.

The test is conducted in the same manner as the first test except in turning irregularly all three parts of the tester.

The ten smoke-glasses give ten shades of each color, and the ten possible relative positions of the diaphragm disc to the color disc multiply the number of combinations by ten. The ten possible positions of the color disc itself further multiply the result. There are thus 1,000 different combinations. It will be necessary to use only 15 or 20 of these, produced at random by the irregular movement of the parts in different directions, provided the person makes no mistakes. Confusions between dark green and blue and between white and yellow are not to be counted as mistakes.

If the subject makes mistakes or hesitates in his answers, a considerably larger number of combinations should be tried and the mistaken answers should be carefully recorded, the true color being written under the name given it.

Any person who calls a white or a yellow by the names "red" or "green," or a red by the names "white," "yellow" or "green," or a green by the names "white," "yellow" or "red," is to be classed as of unsafe color vision.

General suggestions.

While the test is going on, never tell the person whether his answers are right or wrong. If deemed advisable, the mistakes can be told at the end of the test.

In putting on the color disc it is well to vary the side toward the lantern ; this varies the order of the colors.

The following form of record is suggested:

Name of person examined

Age Residence ....

Previous history ....
(Note here anything that may have a bearing on color sense.)

For the left eye the color sense was found to be
(The examiner must answer "safe" or "not safe.")

For the right eye the color sense was found to be ....
(The examiner must answer "safe" or "not safe.")

Test I. Mistakes made:

Test II. Mistakes made:

Test III. Mistakes make:

POSSIBLE FORMS OF COLOR VISION THAT MAY BE MET IN TESTING.

It seems a well-established fact that most persons possess a color system in which any color can be considered as resulting from the union of three fundamental colors, red, green and blue. Such persons are called "trichromats."

If all trichomats possessed exactly the same vision of color, we could establish equations showing how much red, green and blue are required to produce any given color. It is a fact that, if we allow room for considerable variation, such equations can be established for a large portion of the trichromats. Such a group of persons might be said to possess a common "type" of color vision. By this we would not mean that the color vision was exactly alike in all cases but that they varied according

to the usual laws of organic variation around a mean or average form of color vision. This type of color vision, which is that of the majority of human beings, is called "normal trichromasy."

As color vision is one of the properties of an organism, we may expect it to vary as other properties do. The fundamental principle of variation leads us to expect that among trichromats there will be individuals in whom the red, or the green, or the blue sensation is weaker than in the normal trichromats. Such individuals have been found; they may be called "color weak trichromats."

I have had cases of this color weakness among my students. The first one I observed was absolutely perfect in passing the usual wool test but told me, after it was over, that when watching an approaching car carrying a red or a green light, he was unable to tell the color until it had approached within a block or two. When the wools were removed to a considerable distance away, he became completely confused and made judgments like a person in whom the green sensation was weak or lacking.

The cases are not so uncommon or so striking as would at first be supposed. Engineers have confessed—generally regarding some friend—that the wool tests have been passed by men who could not distinguish the signal lights in a fog. Some years ago several letters appeared on the subject in the London "Times" of which I quote a few.[1] One engineer made the following statement: "I have been on the railway for thirty years, and I can tell you the card tests and wool tests are not a bit of good. Why, sir, I had a mate that passed them all, but we had to pitch into another train over it. He couldn't tell a red from a green light at night in a bit of a fog." Another wrote: "To me the colored skeins of the wool test are no test at all, for both reds and greens in all shades are unmistakable; and although I have undergone this examination whenever an opportunity has presented itself, I have never tripped. Close at hand, reds and greens are to me as to other people; at a distance, however, my sense of color, in regard to red especially, is all astray. Standing on the edge of a large field glowing with poppies, I see them up to about thirty or forty yards as other people see them; but beyond that distance they gradually merge into a neutral tint and become lost. . . . A distant red light over a chemist's door appears to be a dull yellow, and the same applies to a red railway signal." Another person writes, concerning his wife: "The red geraniums near my window she could see as well as I could; but those a hundred yards off were lost in the green. She could choose and compare colored

[1] ELDRIDGE-GREEN, Color Blindness, 216, 217, 227, London 1891.

silks or worsteds, and could paint well, and was a remarkable good colorist.'' Such persons can be considered as cases of color-weakness.

In another type of color vision, known as ''abnormal trichromasy'' the red and blue fundamental sensations are apparently the same as those in normal trichromasy but the green sensation is of a different kind.

These abnormal trichromats seem to be quite common. They ave been described by RAYLEIGH, DONDERS and KOENIG.[1]

I have for several years tested my classes in the following way; the origin of the test I have forgotten. A large red and a large green MAXWELL disc are placed in the usual fashion on the axle of a motor; a small black and a small white disc are then placed before the others. The room is darkened and a sodium light is produced by inserting salt (contained in a little platinum basket) in a BUNSEN flame. Both sets of discs appear in grayish yellow. Their proportions are changed until they match. About 70% to 80% of my pupils agree on one match and the rest on a different one. This would seem to indicate a rather large number of abnormal trichromats. It is difficult to make any statement concerning how the world appears to these abnormal trichromats beyond saying that is certainly quite different. KOENIG and DIETERICI's measurements suggest that the preponderance of green is shoved toward the red end of the spectrum. We might infer that for such persons our reds and oranges are more yellow, our yellows more green and our greens more blue to them. This might lead to some understanding of the peculiar colorings used by some artists.

The same principle of variation leads us to assume the existence of cases in which one of the fundamental sensations is entirely lacking. We thus would have ''red blindness,'' ''green blindness'' and ''blue blindness.''

Cases of acquired color blindness are presumably of this form. The excessive use of tobacco and alcohol quite frequently produces color blindness. Accidents, such as severe falls and contusions, may also produce it. Cerebral troubles, over-strain of eyes, and other diseases sometimes result likewise.

In all such cases of acquired color defects we may readily assume the condition to arise by injury of the organ of one or more of the three fundamental colors; it is thus a true ''color blindness'' for one of the colors red, green or blue.

[1] RAYLEIGH, *Experiments on colour*. Nature, 1881 XXV 67.

DONDERS, *Kleurvergelijkingen*, Onderzoek. i. Labor. d. Utrecht. Hoogesch. 1883 (3) VIII 170.

KOENIG UND DIETERICI, *Die Grundempfindungen u. ihre Intensitätsverteilung im Spektrum*, Zt. f. Psychol., 1893 IV 291.

Many persons have been found for whom it was possible to produce all colors by mixtures of two fundamental colors, a "warm" color and a "cold" color. Such persons would be called "dichromats."

Two distinct classes of dichromats are known; they are said to be "of the first class" and "of the second class."

The cold color of the dichromats is the same as the blue of the trichomats, the warm color is, in all probability, yellow, and not, as formerly supposed, either red or green.[1]

The two classes of dichromats differ only in the proportions of yellow found in any color of nature. The spectrum and all nature appear to them as systems of yellow, yellowish white, white, bluish white and blue; particular objects in the two systems differ only in yellowness or blueness. It is inconsistent with the facts to speak of "green blindness" and "red blindness"; the only safe way is to use the terms "dichromasy of the first class" and "dichromasy of the second class," as these will not imply a supposition that may prove incorrect.

It has been suggested that congenital dichromasy may be a phenomenon of atavism, or a return to the condition of color vision at a period in the ancestry of the animal kingdom when only two color sensations were present.

Still another form of color-vision is found in monochromasy. All the visible objects are seen as shades of one color. What this color is, it has as yet been impossible to say. In cases of congenital monochromasy it is undoubtedly not red, green or blue.[2]

We can also expect cases of "double color blindness" to arise from the failure of two of the fundamental sensations. Thus we would have red, or green, or blue monochromasy. Such forms of monochromasy may occur in pathological cases. Other cases of acquired monochromasy seem to follow still different types.[3]

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[1] v. HIPPEL, *Ein Fall von einseitiger congenitaler Roth-Grünblindheit bei normalem Farbensinn des anderen Auges*, Archiv. f. Ophthalmologie, 1880 XXVI (2) 176; v. HIPPEL, *Ueber einseitige Farbenblindheit*, 1881 XXVII (3) 47.

HOLMGREN, *Flere Fall of ensidig Färgblindheit*, Upsala Läkaref. Förh., 1881 XVI 222. Also in Centralblatt f. d. med. Wiss., 1880, 898; Congrès internat. périodique des sciences méd., 8me Session, Copenhague, 1884; Ann. d'Oculiste, 1884 XCII 132.

KOENIG UND DIETERICI, *Die Grundempfindungen u. ihre Intensitätsverteilung im Spectrum*, Zt. f. Psychol., 1893 IV 345.

HELMHOLTZ, Physiologische Optik, 458, Hamburg und Leipzig, 1896.

[2] KOENIG UND DIETERICI, *Die Grundempfindungen und ihre Intensitätsverteilung im Spektrum*, Zt. f. Psychol., 1893 IV 327.

[3] KOENIG, *Ueber den Helligkeitswerth der Spektralfarben*, Beiträge z. Psychol. u. s. w., Helmholtz gewid., Hamburg, 1891.

The principles underlying these views of the nature of color defects can be more precisely stated in the following way.

If the amounts of red, green and blue be laid off on the rectangular axes, X, Y, Z, any color i will be given by

$$i = f(x, y, z).$$

The assumption that the function is of the form

$$i = x + y + z,$$

is recommended by its simplicity. Such a color equation means that any actual color seen can be represented as a compound consisting of certain quantities x, y, z of the three fundamental sensations X, Y, Z. It also implies that the composition is a simple addition of the three fundamentals

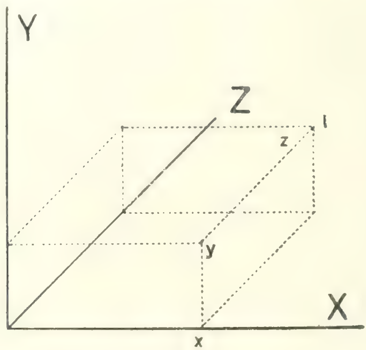

Fig. 6.

without an influence of one on the other. Although this apparently does not hold with exactness for some phenomena of color, yet is valid within wide limits with close approximation.[1]

Such a color equation is represented in Fig. 6.

If r, g, b be taken as the strongest sensations of red, green and blue, obtainable in any person, the color-system for that person will be a pyramid whose base is rgb and whose apex is at the origin of coordinates (Fig. 7).

By supposing that $r=g=b$ we have the right pyramid used by Lambert[2] to indicate the color system. This supposition is, however,

[1] Helmholtz, Physiologische Optik, 370, 2 ed., Hamburg und Leipzig, 1866.

[2] Lambert, Farbenpyramide, Augsburg, 1772.

not a necessary one. In fact, the general character of our whole constitution would lead us to believe that an equality of this sort is not the usual case.

We can justifiably apply to color certain principles that have been found valid for the whole organism. One such principle is that of *variation.* We can, at the start, expect that any property common to a

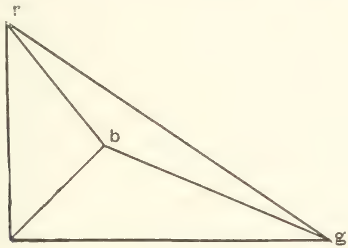

FIG. 7.

group of organisms will be found in various degrees in the various members of the group.

Applying this principle to color, we may say that a given series of colors $i_1, i_2, \cdots, i_n$ derived from the equations $i_1 = x_1 + y_1 + z_1$, $i_2 = x_2 + y_2 + z_2$, $\cdots$ for one type of color vision might require slightly different equations for various individuals. Thus the same colors $i_1, i_2, \cdots, i_n$ might require the equations $i_1 = x_1' + y_1' + z_1'$, $i_2 = x_2' + y_2' + z_2'$, $\cdots$, for a second kind of vision; $i_1 = x_1'' + y_1'' + z_1''$, $i_2 = x_2'' + y_2'' + z_2''$, $\cdots$, for a third kind, etc. We might consider the type $i = x + y + z$ as the standard and express the varying individuals in terms of it by equations of the form $i = \alpha x + \beta y + \gamma z$ where α, β, γ are coefficients changing for each individual.

The individual sets of values for α, β, γ may group themselves around various most frequent values and may represent types of color vision. We may say that $\alpha = \beta = \gamma = 1$ is the expression for the normal type for which the equation is $i = x + y + z$. Any other group of individuals whose equations are similar enough to furnish a typical form may be classed as an abnormal type. All persons belonging to abnormal types might be called "color defectives."

The fundamental principle of variation leads us to assume a type in which α is much less than β and γ. This gives an equation of the form $i = \alpha' x + \beta y + \gamma z$ in which $\alpha' < \beta = \gamma$. The equations $i = \alpha x + \beta' y + \gamma z$ and $i = \alpha x + \beta y + \gamma' z$ would arise likewise for analogous types.

These would be the equations of persons who have been called "color weak."

When the variation is so great that α, β or γ become 0, there arise cases of what is properly called "color blindness." Such persons might be characterized as "red blind" with the equation $i = \beta y + \gamma z$, "green blind" with the equation $i = \alpha x + \gamma z$, and "blue blind" with the equation $i = \alpha x + \beta y$.

Other types of variations of α, β, γ may occur but no data yet exist for stating the facts.

It is also quite possible that the three fundamental sensations may not be the same in all persons. In one class of persons already discovered the fundamental green sensation is different from that of the majority; such "abnormal trichromats" would have equations of the form $i = x + w + z$. The analogous types of abnormality like $i = u + y + z$ and $i = x + y + w$ and also the subordinate types probably also exist but have not been discovered.

The two forms of dichromasy are characterized by equations of the forms $i = w_1 + z$ and $i = w_2 + z$. These also are to be considered only as typical forms from which the individuals vary just as in the cases of trichromasy.

Typical monochromasy is indicated by $i = p$ while the possible cases of double color blindness, or red, green or blue monochromasy, would be expressed by $i = x$, $i = y$ and $i = z$.

The preceding treatment is, as stated, based on the supposition of only three fundamental colors in normal trichromatic vision. If a fourth color is to be added, the treatment would be modified by the introduction of a fourth variable into the fundamental equation on p. 16, with the result

$$i = f(x, y, z, v)$$

which on the usual assumption becomes

$$i = x + y + z + v.$$

If this is the type of vision of the majority of eyes, the persons classed as normal trichromats (pp. 12, 16) are really tetrachromats and those classed as abnormal trichromats (p. 14) are really abnormal tetrachromats. The possibilities of variation in any or several of the fundamental sensations would lead us to expect to find among the forms of defective color vision four types of triple color blindness, $i = x$, $i = y$, $i = z$, $i = v$; six types of double color blindness, $i = \alpha x = \beta y$, $i = \alpha x + \gamma z$, $i = \alpha x + \delta v$, $i = \beta y + \gamma z$, $i = \beta y + \delta v$, $i = \gamma z + \delta v$; four types of single color blindness, $i = \alpha x + \beta y + \gamma v$, $i = \alpha x + \beta y + \delta v$,

$i = \alpha x + \gamma z + \delta w$, $i = \beta y + \gamma z + \delta w$, with numerous types of monochromasy, dichromasy and trichromasy in which several colors are combined in one fundamental sensation w (p. 15).

Only a few types of color vision are at present accurately known and a decision concerning the three or the four color theory cannot yet be definitely made.

In conclusion we may repeat that the forms of color vision are merely types of groups of individuals, that the members of a group vary from the type and that more extended examination of unusual cases would probably show most of the intermediate steps between the typical forms.

In the midst of such diversites in the types of color defect it would be hardly safe to base any test on the supposition that the prevailing defect is of any one or two types. To suppose that all essentially important cases of defect belong to the two types of dichromasy known as the first and the second would be a neglect of somewhat numerous other types. Such a supposition is used as the basis for the HOLMGREN wool test; it is evident that the various forms of "color weakness" and the like are not provided for. Furthermore, the HOLMGREN test is based on the additional supposition that the two forms of dichromasy are "green blindness" and "red blindness"—a supposition that is unquestionably unjustifiable.

Under these circumstances the only safe method of procedure in selecting a test seems to lie in avoiding all suppositions while making the test conform as closely as possible to the conditions of actual practice.

CHARACTERISTICS OF THE COLOR SENSE TESTER.

1. The forms of color defect are so numerous and so various that a reliable test must be one that imitates the conditions found in the actual exercise of color vision. The color sense tester imitates these conditions closely.

2. This imitation of the practical conditions extends to the kind of object used, namely, colored lights under different conditions of brightness; to the kind of decision required, namely, the character of the light; to the kind of mental operation required, namely, a judgment after comparison.

3. The colored glasses are unchanged by use or time; the test is a constant one and needs no readjustment or renewal.

4. The performance of the test requires only a few minutes.

5. The instrument can be used in a portable form. Employees can be tested on shipboard or on a train.

6. The test is absolutely automatic. The decisions of the person tested

are recorded on the prepared blank. A comparison of the results with a list of the actual colors will render the decision immediately. No call is made upon the intelligence or color vision of the examiner; it is only required that he shall record the answers honestly.

RESEARCHES ON MOVEMENTS USED IN WRITING

BY

Cloyd N. McAllister, Ph.D.

The experiments to be reported here were carried out during the academic year 1899–1900.

I. Relative ease of upward and downward movements.

This investigation was begun by a series of experiments arranged to determine the relative ease of movements of abduction and those of adduction. It was thought that the ease would be shown by the rapidity of the movement.

For convenience in describing the movements made, the usual mathematical conventions were adopted. If the intersection of the *X* and *Y* axes be the center about which a radius may revolve, the circle generated will be divided by the axes into four quadrants. When the moving radius is coincident with the right-hand portion of the *X* axis, it is said to be in its initial position; the angle it now makes with this part of the *X* axis is 0°. As it is revolved counter-clockwise the angle it makes with its initial position is increased until it gets back to its initial position again, when it may be said to have generated an angle of 360°; the quadrants of the circle are named I, II, III and IV, in the order in which the moving radius has passed through them (Fig. 1).

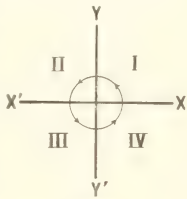

Fig. 1.

Movements of the right arm in writing which follow the direction of the radii of quadrant I are movements of abduction, those of quadrant III are movements of adduction.

Most of the movements used in writing are those which may be defined as movements following the direction of the radii in quadrants I and III. All the old systems of stenography use movements in quadrant IV to a great extent, but the movements in quadrant II are rarely, if ever, used. The backward movements in stenography which sometimes appear to lie in quadrant II, are arcs of a circle or of an ellipse, made by a backward swing.

A. Large movements.

First series of experiments.

The apparatus involved, first, a pair of contacts previously used for measuring the velocity of arm movements.[1]

On a horizontal rod, forty centimeters long, the ends of which were clamped to standards made of a vertical rod fastened in a heavy iron base, were placed two metal blocks, *D* and *D'* of Fig. 4. Each of these blocks carried a light bamboo rod, *C* and *C'*, held in a vertical position, in small revolving clamps. A slight touch on one of these rods caused it to fall, and in so doing, to make an electrical contact for an instant. The rod could be made to fall in one direction only. These two contacts were arranged parallel to each other and in series with a Deprez marker. The distance between them was such that the hand used to knock down the rods must move through a distance of ten centimeters from the time the first contact was made until the second one was made.

Two horizontal rods, one on each side of the perpendicular rods whose movement formed the contacts, and at a distance of one and three-tenths centimeters below their tops, when they were upright, acted as a rest for the finger before beginning the movement, and as a guide during the movement. By this means all the subjects were compelled to move the hands in the same direction and in the same horizontal plane. It was not intended that the hand should bear on these rods in the movement, and this did not occur. In moving the hand rapidly from rest, the tendency was to raise it slightly and sometimes it was raised enough to be carried over the top of the second bamboo rod, not touching it at all. This occurred when the subject permitted his attention to lag.

The subject stood in a comfortable position before a desk that could be adjusted so that the lines of the movement to be made, lay in a horizontal plane slightly below the elbow. This position of the plane of the movements was found necessary in order to permit a free movement of the whole arm.

A large cardboard was placed on the top of the desk and lines drawn thereon to mark the positions for which the apparatus must stand for the angles 0°, 15°, 30°, 45°, 60°, 75° and 90° of quadrant I, and 180°, 195°, 210°, 225°, 240°, 255° and 270° of quadrant III; the purpose of which was that the apparatus might be in the same relative position with the body in all of the movements, and for all the subjects. In these two

[1] Scripture, *New apparatus*, Stud. Yale Psych. Lab., 1895 III 106; also The New Psychology, 166, London 1897.

quadrants the apparatus had the same relative positions for the corresponding angles; the end that was near the body for quadrant I was away from the body for quadrant III. The subject was required to make the movements with the palm of the hand down, the hand closed, except that the forefinger was extended.

Movements in quadrant I, were easily made without any tendency on the part of the subject to increase the speed by the aid of movements of the finger. In the latter part of quadrant III, however, it was found that either the wrist would have to be turned in an awkward manner toward the body in order to move the bamboo rods, or the palm of the hand would be turned toward the body, thus allowing the finger to take the form of a hook with which to pull down the bamboo rods; in this position a movement of the finger, like that of closing the hand, increased the speed of the movement.

A paper cartridge shell was used as a cap for the finger. A wire soldered on the brass head of the shell formed a hook; slits in the paper tube of the shell permitted it to be used by subjects with a large finger; a small rubber band around the tube caused the sections to lap and so fit closely to the fingers of the subjects having small hands; it also bound the cap so closely to the finger, that it did not slip during the movement. The subject had this cap on his finger during the entire experiment.

The apparatus was so placed that the middle point of the distance between the bamboo rods was on a line perpendicular to the front of the body, and half way between the middle of the body and the right side. Its distance from the body was constant, except for the slight oscillating movements of the subject during the experiment. The distance was such that the elbow hung freely by the side of the body when the finger was in position to begin the movements of quadrant I, or at the end of the distance measured for the movements in quadrant III.

Each time the electrical contact was made by the movement of one of the bamboo rods the marker recorded the fact on a drum, which, for convenience, was turned by a small electric motor. The drum used is one previously described by Dr. Scripture.[1] On the large base supporting the drum is an upright standard which may be moved along the drum by means of a screw turned by the hand. A bed carefully planed and parallel to the axis of the drum guided the movement of the standard.

A fork interrupted the current from a four-ampère battery 100 times a second. This was made to pass through the primary coil of a spark coil. The secondary coil was connected at one pole with the base of the drum,

[1] SCRIPTURE, The New Psychology, 85, London 1897.

and at the other pole with the marker. The line produced by the marker was in this way divided by sparks, the distance between the successive sparks representing the time of one vibration of the fork. The number of such spaces between the two records made by the marker for each movement of the arm, gave the number of hundredths of a second consumed in the movement. With some practice it became quite easy to divide these spaces by the eye into tenths, thus permitting the record to be given in thousandths of a second. A specimen record indicating sixty-two thousandths of a second is shown in Fig. 2.

The subject was requested to place his right hand so that the first finger, with the cap upon it, should rest on the horizontal bars close to the

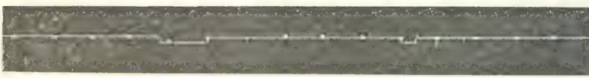

Fig. 2.

first upright bamboo rod, but not touching it; thus the forearm and hand formed a straight line, the elbow hanging freely by the side of the body.

At the signal "ready" he prepared himself to make as rapid a movement as possible in the direction of the line joining the tops of the bamboo rods, moving the joints of his elbow and shoulders only, and allowing his elbow to hang close to the body. At the signal "go" he executed the movement.

Care was taken to see that the distance at which the finger was placed from the bamboo rods before beginning the movement did not vary for the different individuals. When each subject, in the several preliminary trials before a record was taken, indicated the distance he would most naturally use, he was cautioned that a change of this distance would decrease or increase the time required to pass through the distance between the two rods, according as he began the movement with the finger farther from, or nearer to, the first rod; and in order that his record might be of value to us he must aid us in keeping this condition the same throughout the experiment. This distance did not exceed one centimeter.

When the signal "ready" was given, the marker was moved slowly along the drum until after the movement was executed by the subject; the movement of the marker was then stopped; the subject raised the bamboo rods to the vertical position and placed his hand in position for making another movement.

Five movements were recorded at intervals of 15° throughout quadrant I, beginning at 0°. Five records were then taken for corresponding angles in quadrant III, beginning at 180°, and then beginning at 270° and reversing the order, thus obtaining ten records for each angle used in quadrant III. Five records more were then taken at intervals of 15° throughout quadrant I beginning at 90° and reversing the order of taking the first records. Ten records were thus obtained for each angle used in quadrant I.

Each subject removed his coat and right cuff before beginning the experiment, so that the sleeve or cuff might not strike against the standards or guiding rods. But even with this precaution there was found some difficulty in making the movements in the latter parts of both quadrants. The subjects complained that the apparatus interfered with a free movement.

Records were taken from eight persons with this apparatus. Two of the subjects, however, changed their positions in standing and the position of the hand with reference to the first rod to such an extent that it was decided to make no use of their records. Of the other six persons, the average of whose records are given in Table I, two, *A* and *B*, are Japanese, and two, *C* and *D*, are American students in the graduate department of this university. Their ages are between twenty-three and thirty years. One, *E*, is a freshman in the academic department, whose age is nineteen years, and one, *F*, is a college graduate, at present superintendent of a department in a large factory in this city; his age is thirty-four years.

The columns in Table I that are headed *P. E.*, contain what may be called the "immediate probable errors." These are calculated by the usual formula

$$P.\ E. = \tfrac{2}{3}\sqrt{\frac{v_1^2 + v_2^2 + \cdots + v_n^2}{n - 1}}$$

where v_1, v_2, $\cdots$, v_n are the differences between the individual measurements and the average, and n is the number of measurements. This value is a measure of the uncertainty of the subject.

The probable error of the average, or "final probable error" is obtained by dividing the immediate probable error by $\sqrt{n}$. This indicates that the subject may be expected with a probability of $\frac{1}{2}$ to give results in a series of measurements that would not exceed his $Av. + \frac{P.\ E.}{\sqrt{n}}$, nor be less than his $Av. - \frac{P.\ E.}{\sqrt{n}}$.

Table I.

First quadrant.

	0°		15°		30°		45°		60°		75°		90°	
Subject	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*
A	31.3	5.1	20.1	2.2	27.9	2.0	30.3	6.6	45.5	3.4	42.6	2.0	45.7	3.4
B	57.7	8.5	45.2	3.0	40.0	4.2	40.2	5.2	67.2	13.0	90.9	8.6	93.3	5.8
C	88.1	8.0	82.8	0.7	95.8	8.5	109.3	13.1	95.1	6.8	105.0	6.3	110.1	11.8
D	20.6	3.0	28.4	1.7	20.0	2.8	20.1	1.5	31.8	2.1	45.0	5.1	45.5	2.4
E	57.9	4.6	65.7	5.2	83.7	8.4	82.4	4.5	89.4	6.1	87.0	7.2	98.0	8.4
F	64.9	18.1	49.4	6.8	48.3	5.7	53.7	4.8	56.8	3.6	57.9	6.1	68.0	7.2
Av.	54.8	8.0	44.6	4.8	54.8	5.6	58.8	5.9	64.3	5.8	71.4	5.9	76.8	6.5

Average for quadrant I, 60.7; average probable error, 6.1.

Third quadrant.

	180°		195°		210°		225°		240°		255°		270°	
Subject	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*
A	37.0	3.3	32.7	1.9	33.7	1.7	35.1	0.6	38.4	3.1	42.9	5.1	47.5	4.5
B	46.8	2.7	55.8	5.6	55.7	4.5	57.9	6.1	62.8	4.6	67.7	8.7	79.9	7.7
C	107.0	14.4	94.4	9.5	103.6	16.5	98.6	9.5	109.1	11.2	100.0	7.1	107.0	7.0
D	27.4	2.6	28.4	4.3	26.9	0.7	26.1	1.7	31.8	0.7	45.0	1.5	45.5	1.7
E	76.6	7.7	74.7	12.3	89.7	5.0	92.3	5.8	100.0	13.5	100.9	6.4	107.8	9.5
F	52.9	1.9	50.8	4.2	51.0	3.6	54.6	3.5	59.7	4.1	65.0	4.8	68.8	4.7
Av.	68.0	5.2	50.3	6.1	60.0	5.6	61.4	5.6	67.2	6.6	69.1	5.6	74.6	6.6

Average for quadrant III, 65.2; average probable error, 5.6.

Unit of measure, $\sigma = 0.001^s$. *Av.*, average of 10 experiments. *P. E.*, immediate probable error.

It will be seen from the table that, with the full arm, and under the conditions of our experiment, the movements in quadrant I are made more rapidly than those in quadrant III, except for the angles of 75° and 90°; the movements at the angles of 255° and 270° of quadrant III are more rapid.

The probable errors for movements of quadrant III are, on the average, smaller than for those of quadrant I. This seems to indicate that though these movements are less rapid, they are more regular. In quadrant I, subjects *A* and *E* do not follow the general law as expressed by the curve, namely, that the time required to make the movement for the angle of 15° is less than that required for 0°. This lack of conformity to the general rule is noticed in quadrant III in the record of subject *B*.

If a line be drawn on the horizontal plane that lies just below the elbow (not more than two and one-half centimeters below) so as to form an angle of 15° with the intersection of this plane with the plane of the front of the body, this line will indicate the direction of the movement

that can be made with greatest ease by the right arm, under the conditions of the experiment. The movement along that line toward the body, being a little slower than the movement from the body.

The results in Table I are shown graphically in Fig. 3. The abscissa denotes the relative size of the angles; the ordinate shows the relative

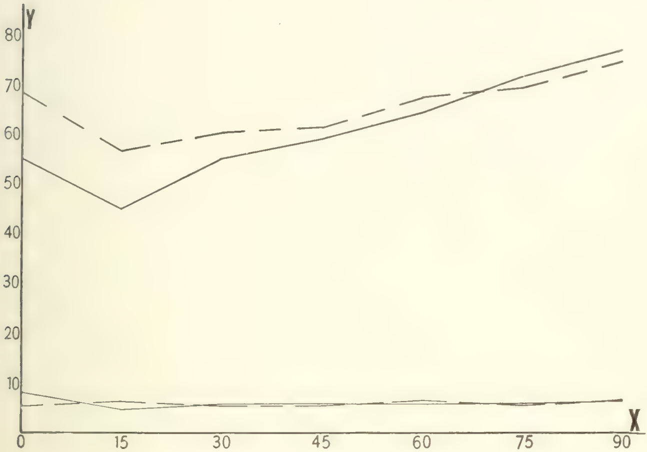

FIG. 3.

time. The dotted lines show the results of quadrant III; the solid line those of quadrant I. For the sake of comparison, the movements of quadrant III are represented as movements of adduction along the radii of the corresponding angles of quadrant I.

The two upper curves show the average time consumed in the movements, the lower curves the probable errors.

The curves of the probable errors show them to be nearly constant; the variations are small.

Second series of experiments.

The following change in the apparatus was planned. Well seasoned white pine was glued together in three thin layers—the grain of the two outside layers being parallel, and that of the middle layer being at right angles to this direction. From this a disc was turned, *A* of Fig. 4, thirty-three centimeters in diameter. A disc, *F*, of pine one centi-

meter thick was cut of the same size as the above and fastened to that one by means of four posts, G, two centimeters by two and one-half centimeters and ten and one-half centimeters long. Two of these posts were placed with their centers diametrically opposite; the other two were placed with one edge on the diameter perpendicular to the diameter joining the first two posts.

By gouging out a small groove across the second pair of posts, on the side in a line with the diameter, it was possible to fasten to them the rod, E, which held the metal blocks, D and D', carrying the bamboo rods, C and C', so that it would hold the bamboo rods directly over this diameter.

The turned disc was used for the upper one and in it was cut a slit, B, nine-tenths of a centimeter wide and twenty-three centimeters long. Its

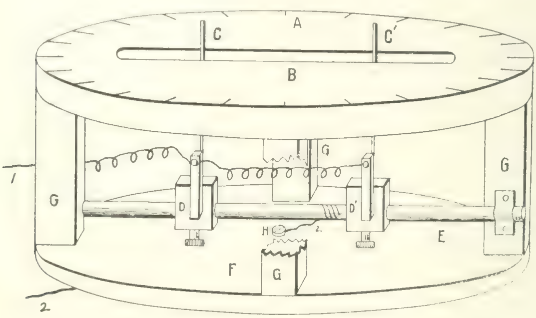

FIG. 4.

middle point is at the center of the disc. This slit being placed over the line joining the bamboo rods, permitted the rods to be raised and lowered with perfect freedom. The rods were made to extend one and three-tenths centimeters above the top of the disc when standing upright.

A wire in the form of a rectangle, not shown in the figure, sixteen and one-half centimeters long and three and one-half centimeters wide, with a wire rod soldered across it parallel to the ends and ten centimeters from one end, was fastened under the top disc by means of guides on each side so that it might be drawn backward and forward under the slit. A small rubber band fastened to the end of the rectangle ten centimeters from the cross rod, and to the disc near its circumference and in line with the slit, held the frame near that end of the slit and against two small nails that were used as stops.

The bamboo rod which must be struck first when standing was but a short distance from the end of the frame nearer the cross rod; but, when it was caused to fall, it fell in the direction of the cross rod, and, although it fell completely below the surface of the disc, was prevented by it from falling down entirely. The other end of the frame performed this service for the other bamboo rod.

By means of a cord running through a screw eye the frame could be drawn toward the other end of the slit, raising the bamboo rods as it moved until it was stopped by two nails in a position such that the bamboo rods were standing upright and ready to make another record. The cord being released, the rubber band returned the frame to its former position where it was ready to catch the bamboo rods again.

The top part was removed from a school desk and in its place there was put a flat desk open in front and back. The top was of the same size as that of the original desk, when the lid was raised to make a flat desk of it, extending sixty-six centimeters from right to left and fifty-three and one-half centimeters from front to back. A circular hole was carefully cut in this top of a size to permit the turned disc to revolve with very little friction. The center of this hole was thirty-three centimeters from the ends and from the front of the desk top. The depth of the desk was such as to permit the other disc to rest on the bottom, while the desk and the turned disc were flush on top. A bolt, *H*, through the center of the lower disc and the bottom of the desk served as an axis for the discs to turn upon. With the aid of a bicycle wrench the desk could be raised or lowered. The chair accompanying the desk was also adjustable.

With the apparatus in this condition each subject could be seated comfortably, and the height of the desk could be made suitable for him. The subject was requested to sit in an upright position; the desk was adjusted so that the top was slightly below the elbow.

The subject reacted to the signals as before, but his whole attention was given to knocking the rods down; they were raised into position each time by the observer by means of the device explained.

The contacts were connected as before, parallel to each other and in series with the DEPREZ marker. The battery wires are indicated by *1* and *2*. The middle point of the distance between the two bamboo rods was at the center of the discs.

The top disc was divided into angles of fifteen degrees; these angles were marked with their values from 0° to 345°, according to the mathematical convention mentioned above; a mark on the top of the desk at the edge of the hole made it possible to adjust the disc for any angle desired.

With the apparatus arranged in this manner records were taken on ten persons for the same angles, and in the same order as in the preceding experiment. Three of these subjects, *A*, *D*, *K*, were graduate students; one, *I*, a senior; two, *J* and *L*, freshmen; two, *F* and *M*, mechanics; one, *G*, a teacher in a commercial college; one, *H*, a high school student. Table II contains the averages for these subjects.

Table II.

First quadrant.

	[illegible]		[illegible]		[illegible]		45°		60°		75°		90°	
Subject	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*
A	59.1	5.0	52.6	3.5	42.1	2.5	46.6	4.1	52.2	9.3	56.6	15.2	66.1	10.6
D	52.7	3.1	46.2	2.7	44.1	2.2	40.8	2.5	42.2	2.5	48.7	3.1	57.3	5.3
F	92.4	7.8	86.3	8.1	85.3	6.6	73.7	6.1	75.9	5.0	78.9	3.6	81.0	4.9
G	73.6	6.6	64.8	8.4	51.3	7.5	60.2	2.9	80.9	9.0	88.5	6.1	93.2	6.7
H	56.3	9.1	48.4	3.9	40.3	4.0	50.5	5.7	42.3	2.7	45.8	2.5	42.0	2.5
I	89.4	6.4	86.9	13.1	76.1	13.7	73.7	9.9	90.8	17.6	77.2	8.2	77.0	9.3
J	76.2	8.4	77.4	6.2	68.5	7.7	53.4	7.0	55.5	6.5	58.5	6.4	60.4	2.6
K	93.1	12.6	83.2	13.5	76.8	5.5	80.8	5.0	73.0	2.7	83.4	6.7	81.4	6.1
L	53.9	5.4	50.1	2.7	43.4	3.1	43.4	2.9	44.5	4.5	49.1	5.7	47.8	4.4
M	95.1	7.6	80.2	4.3	69.5	2.4	66.8	4.3	71.1	5.9	76.9	5.6	88.4	5.6
Av.	74.2	7.3	67.6	6.6	59.7	5.5	59.0	5.0	62.7	6.6	66.4	6.3	69.5	5.8

Average for quadrant I, 65.4; average probable error, 6.1.

Third quadrant.

	180°		195°		[illegible]		225°		240°		255°		270°	
Subject	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*	*Av.*	*P. E.*
A	66.4	9.4	53.7	5.3	49.8	4.5	53.2	6.3	46.1	3.5	51.3	6.9	75.7	11.8
D	35.1	2.8	31.9	2.6	35.1	3.1	37.3	3.6	38.8	2.3	38.8	2.4	51.7	2.8
F	63.4	2.9	68.2	9.8	59.7	4.9	55.4	3.1	63.6	7.0	61.3	4.4	66.9	3.5
G	46.6	2.3	48.8	6.8	46.7	3.3	47.4	2.7	49.0	2.5	59.4	5.3	65.1	4.7
H	37.7	6.3	34.0	2.3	35.0	2.7	38.5	3.7	38.9	2.5	45.1	3.5	52.1	3.7
I	58.6	5.7	62.2	4.3	60.5	8.2	72.2	5.8	70.8	10.9	69.5	6.6	74.0	4.6
J	45.8	5.4	48.0	4.3	50.4	4.9	46.5	4.3	48.5	5.7	54.2	7.4	61.2	6.5
K	74.2	5.7	64.2	5.5	62.3	7.5	60.5	5.2	58.5	7.9	61.6	3.9	56.2	3.3
L	35.3	1.6	29.8	1.6	28.4	2.2	27.7	1.0	33.4	0.8	37.3	2.7	50.5	3.1
M	91.7	10.7	67.6	4.7	64.3	3.3	70.9	5.4	73.4	7.4	95.2	7.1	90.6	5.6
Av.	55.5	5.3	50.9	4.7	49.2	4.5	50.9	4.1	52.1	5.1	57.4	5.0	64.4	5.0

Average for quadrant III, 53.9; average probable error, 4.8.

Unit of measurement, $\sigma = 0.001^s$. *Av.*, average of 10 experiments. *P. E.*, immediate probable error (p. 25).

This table shows a difference of 11.5σ between the averages for all the angles of each quadrant in favor of movements in quadrant III. These

movements require but 82% of the time necessary for the movements in quadrant I. The probable error is 8.9% of the average result for quadrant III and 9.6% of the average result for quadrant I; so that the movements of quadrant III are not only more rapid but also more regular. The control over these movements must then be somewhat more in quadrant III than in quadrant I.

The greatest importance for this investigation lies in the relative rapidity, rather than in the absolute speed; but it may be of interest to note here that the probable error of the determination varies between 3^{σ} and 5.5^{σ}.

In Table I the angles of greatest speed of the two quadrants were corresponding angles of the quadrants. In Table II that is not the case.

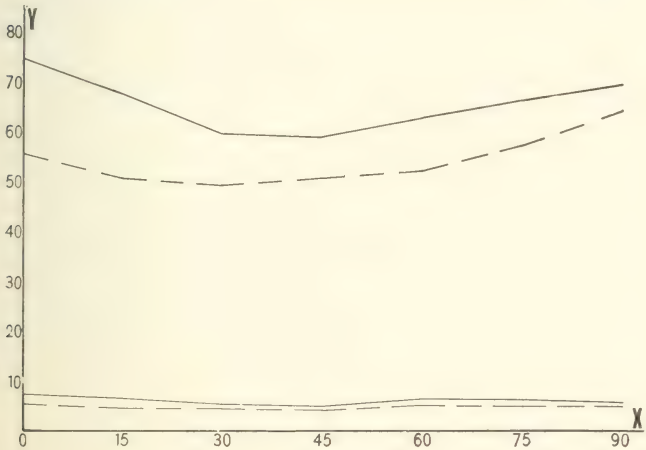

Fig. 5.

In quadrant I the greatest speed was attained at the angle of 45°; in quadrant III at the angle of 210°.

These results are shown graphically in Fig. 5. As in Fig. 3 the movements of quadrant III are represented as movements of adduction along the radii of the corresponding angles of quadrant I. There is here no crossing of the curves. A glance is sufficient to show that the movements of adduction require less time than the corresponding movements of abduction.

The dotted lines show the results of quadrant III; the solid lines those of quadrant I.

The two upper curves show the average time consumed in the movements, the lower curves the probable errors.

The results in quadrant I seem to indicate that the lowest point of the true curve showing the speed of movement in this quadrant must be between 30° and 45° ; in quadrant III, however, the lowest point of the curve would seem to be at just 210°. The lines of greatest speed of movement in the two quadrants when extended, though they do not coincide, are at most not more than 10° apart.

The number of the experiments is not great enough to warrant any positive statements as to what the true law of the motion may be. It is natural to suppose that, were the experiments more extended, it would be found that the angles of greatest speed in the two quadrants are corresponding angles, as was found in Table I. It must be noted that the most rapid movements are not in the middle of the quadrant, but rather at the end of the first third of the quadrant.

The curve of the probable errors is also shown in Fig. 5. The variation in the probable errors is small and they will hereafter be considered as constant.

Table II shows that the probable errors of quadrant I average 6.1^σ, or 9.6% in the average time for all the angles ; those of quadrant III average 4.8^σ or 8.9% of the average time for all the angles.

This difference in favor of quadrant III shows that the movements of adduction are more to be relied upon ; they are not only more rapid but are easier to make.

The form of the curve attracted attention immediately, and Dr. Scripture suggested that it might be a sinusoid. In order to determine the complete curve, records were taken on ten subjects in all four quadrants—the apparatus being the same as before.

Third series of experiments.

The subjects experimented upon in this series were *E*, *G* and *J* of the preceding series, and *N*, *P*, *Q* and *R*, students of the graduate department ; *O*, a boy of fourteen years, and *S* and *T*, mechanics.

In this series, five records were taken for each angle marked on the disc, beginning at 0°, and then five more, reversing the order of the series.

The averages for the different angles for each subject are given in Table III.

TABLE III.

Subject	0°	15°	30°	45°	60°	75°	90°	105°	120°	135°	150°	165°
E	84.0	70.3	78.5	76.2	76.3	79.7	87.0	86.5	94.5	94.0	101.4	87.5
G	69.2	51.8	45.6	46.9	43.3	61.9	76.1	82.6	85.8	78.5	74.2	65.5
J	55.5	51.0	49.0	60.2	73.7	72.0	72.8	80.7	74.4	79.3	69.7	63.0
N	50.3	39.2	34.6	34.9	40.5	46.9	51.0	58.8	59.0	59.2	58.2	53.0
O	60.6	57.7	57.7	44.8	49.4	50.3	52.7	64.5	72.8	77.9	72.4	63.5
P	55.5	55.4	51.5	47.3	54.2	55.3	62.3	62.7	65.8	60.6	61.7	46.9
Q	66.0	63.4	55.2	65.1	50.7	61.6	65.3	67.7	81.6	82.1	75.8	78.1
R	53.1	46.0	35.1	30.4	38.4	37.3	65.7	64.8	67.6	82.3	70.5	68.1
S	48.5	41.2	36.0	32.7	32.2	43.8	49.3	52.5	58.8	54.3	45.2	34.5
T	37.4	32.3	28.5	26.0	25.1	28.7	34.4	34.4	38.8	42.8	38.7	32.8
Av.	58.0	50.8	47.2	46.6	48.4	53.8	61.7	65.5	69.9	71.1	66.8	59.3

Subject	180°	195°	210°	225°	240°	255°	270°	285°	300°	315°	330°	345°
E	64.4	64.2	60.0	62.4	64.8	62.7	80.9	92.3	97.4	89.9	90.5	74.4
G	49.6	45.1	40.9	43.8	40.1	47.7	57.6	59.5	61.0	71.1	74.1	69.0
J	58.0	55.6	50.5	61.0	50.1	65.3	59.8	68.0	55.3	72.4	82.6	74.0
N	49.4	37.0	31.4	31.7	35.2	37.8	48.8	50.4	56.3	57.4	55.8	59.8
O	52.0	47.7	47.7	47.0	45.5	53.2	53.1	63.1	76.8	75.4	77.2	75.0
P	42.5	42.2	49.8	49.1	58.2	62.3	56.2	66.1	73.7	71.0	68.3	68.7
Q	55.0	55.0	53.3	52.5	53.1	61.5	68.3	75.3	83.6	84.7	88.4	78.5
R	69.7	36.5	33.7	42.2	37.6	37.8	40.2	45.5	45.8	53.8	60.7	64.2
S	31.4	33.1	32.3	35.5	40.3	43.1	48.8	48.7	50.1	50.4	47.7	44.4
T	26.5	22.1	25.0	24.1	24.9	29.3	30.4	32.8	36.6	41.1	40.1	40.7
Av.	49.9	43.9	42.5	44.9	45.6	50.1	54.4	60.8	63.7	66.2	68.5	64.9

Average for quadrants I and II, 58.3; average for quadrants III and IV, 54.6; average for all angles, 56.4.

Unit of measurement, $\sigma = 0.001^s$.

The results are very much such as might be expected from a study of Table II.

The average for all of the angles is 56.4^σ, while that of quadrants I and II is 58.3^σ and quadrants III and IV is 54.6^σ. Movements in quadrants

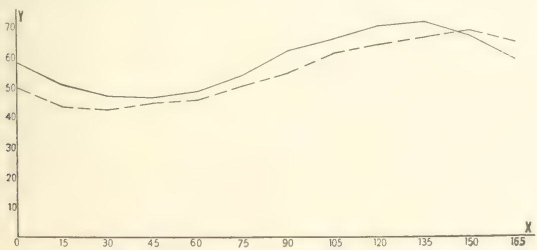

FIG. 6.

III and IV are, then, easier than those in quadrants I and II or, in other words, movements toward the body are more rapid than those from the body.

Here again it is noticed that the most rapid movements are a little before the middle in the quadrants I and III.

For a complete comparison of movements toward the body with those from the body in all directions, the movements in quadrants III and IV may be considered as movements toward the body along the radii of quadrants I and II, respectively, and the results plotted as shown in Fig. 6. The abscissa then shows the relative size of the angles of quadrants I and II; the ordinate the time consumed in the movement.

The dotted line shows the results for quadrants III and IV; the solid line those for quadrants I and II.

In Fig. 7 the movements are considered as independent. The abscissa shows the size of the angle from 0° to 345°. The solid line in this figure shows the results of Table III.

The curve of results as shown in Fig. 7 would indicate that the lowest point in quadrant I must lie between 30° and 45°, and in quadrant III very near 210°.

The highest point of the curve is at the angle 135°, or the middle of quadrant II; but in quadrant IV the highest point is at 330°, or is beyond the middle of this quadrant.

The similarity to a sinusoid is immediately noticed in the curve, with slight distortion to the right. This distortion suggested that the element of practice or fatigue might have entered because of the manner of taking the records in so long a series.

Naturally the speed of the subjects would improve after the first few angles, due to practice, and, along toward the close of the hour required to take the record, fatigue might have shown itself in a diminished speed, especially in quadrant I, where the least practice would be shown.

Fourth series of experiments.

To eliminate this error due to practice or fatigue, if it existed, it was decided to take records on ten persons, for the same angles, but to vary the order. The order chosen for the first five records on each angle was 0°, 90°, 180°, 270°, 15°, 105°, and so forth, that is the angles formed by the moving radius when coincident with the X and Y axes, or the beginning of each quadrant; then for the four angles that are 15° larger, respectively, than the four angles just taken, and so on, to the angle 345°; for the next five records this order of the angles was reversed.

The subjects were *G*, *O*, *P*, *Q*, *R* and *S* of the preceding list, *A* and *F* of series one and two, *H* of series two, and *U*, the janitor of the laboratory.

The results are shown in Table IV.

TABLE IV.

Subject	0°	15°	30°	45°	60°	75°	90°	105°	120°	135°	150°	165°
A	56.4	51.0	54.8	51.2	42.0	59.8	65.9	71.9	75.3	78.9	82.0	44.7
F	87.4	80.6	57.9	61.5	67.1	71.3	69.9	75.6	78.8	74.8	88.6	95.5
G	47.8	44.7	36.8	41.1	46.9	49.9	62.0	77.1	74.9	75.2	75.6	54.3
H	50.1	46.1	38.5	37.7	45.2	48.9	52.6	48.3	51.2	51.1	44.0	37.2
N	60.7	58.1	55.5	48.9	52.0	55.7	71.6	72.3	73.9	68.2	70.2	59.8
O	55.9	48.5	43.2	48.7	47.2	55.5	61.3	61.3	67.7	66.7	67.8	56.6
P	54.9	48.1	47.4	50.3	42.0	52.8	53.3	58.4	69.9	63.5	61.7	46.6
Q	59.4	49.3	46.4	44.1	52.2	65.1	74.1	77.8	81.4	80.5	71.8	70.5
R	41.4	41.7	37.1	34.0	32.4	40.4	46.6	46.6	57.6	59.5	58.8	50.1
S	59.3	55.6	48.8	51.1	43.3	50.7	52.4	66.8	71.1	84.9	69.3	68.9
Av.	57.3	52.4	46.6	46.9	47.0	55.0	61.0	65.6	70.2	70.3	69.0	58.4

Subject	180°	195°	210°	225°	240°	255°	270°	285°	300°	315°	330°	345°
A	45.0	43.4	53.4	56.6	54.3	51.7	59.1	62.7	69.3	72.8	66.9	66.8
F	57.6	48.3	52.5	59.9	55.0	57.2	69.4	78.5	81.4	80.3	88.4	86.0
G	44.9	43.9	38.8	43.3	47.0	54.8	58.1	60.0	65.0	67.0	75.2	63.9
H	36.8	35.1	38.2	41.0	43.1	47.7	46.2	49.7	55.7	58.0	55.3	53.8
N	42.8	46.1	45.4	49.2	51.8	54.5	64.4	62.1	67.6	67.9	63.7	64.0
O	44.3	41.2	43.1	47.3	50.1	53.0	58.2	61.9	63.1	69.2	57.7	65.0
P	42.4	36.0	38.9	47.5	39.6	46.4	50.2	60.0	59.8	64.1	61.7	59.9
Q	42.5	42.1	49.5	48.9	52.7	54.5	63.7	63.7	63.9	62.2	65.4	59.7
R	30.2	29.2	29.5	36.1	38.1	44.2	46.6	53.4	55.7	58.4	50.5	55.2
S	42.3	45.2	43.3	42.6	47.6	45.5	60.8	69.7	65.4	70.4	75.6	62.8
Av.	42.9	41.1	43.3	47.2	48.0	51.0	57.7	62.2	64.7	67.1	66.0	63.7

Average for quadrants I and II, 58.3; average for quadrants III and IV, 54.6; average for all angles, 56.4

Unit for measurement, $\sigma = 0.001^s$.

The individual results do not correspond exactly with those of the preceding table, but the averages for quadrants I and II, for quadrants III and IV, and for all the angles together, are respectively equal. In both tables they are 58.3^σ, 54.6^σ and 56.4^σ, respectively.

Fig. 7 shows the results of this table graphically. For the sake of comparison, the results of Tables III and IV are shown together in one figure.

The dotted line in this figure shows the results of Table IV.

The regular crossing and recrossing of the curves of Tables III and IV is very noticeable in quadrants I and II. The influence of fatigue and

practice that it was thought might be eliminated, if actually gotten rid of, was not sufficient to account for the declination of the axes toward

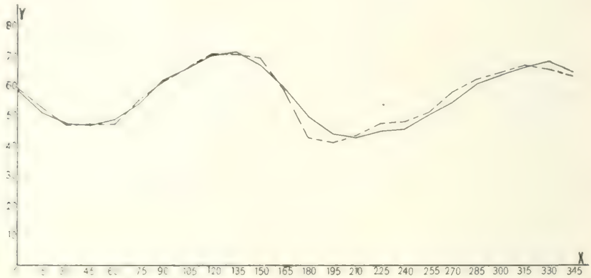

Fig. 7.

the right, for there is even more declination in the curve of Table IV than that of Table III.

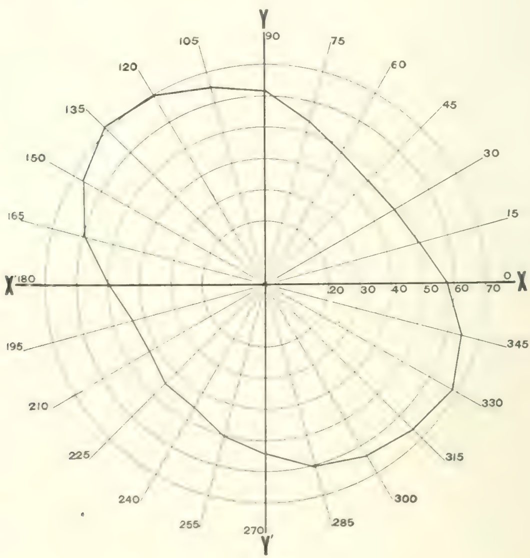

Fig. 8.

It seems to be beyond question that the free full arm movements in a horizontal plane—the forearm being horizontal—are made more rapidly toward the body than away from it.

For the sake of presenting the results of Table III to the eye in a manner such that it will be possible to fix in mind the relative speed of movements in the various angles in which they were made, the results have been plotted on a circle in Fig. 8. The distances along the radii are proportional to the time required to make the movement in that direction.

As this figure lies before the reader, he can see at a glance, in what direction the movement is most easily made.

If quadrant I be taken as the standard, we find that quadrant II requires for the average of all movements, 30% more time; quadrant III, 10% less time, and quadrant IV, 25% more time.

The slowest movements are in quadrant II.

Fifth series of experiments.

The results found in the preceding experiments indicate the directions in which we might expect the hand to move if one were asked to make some long strokes with a pen; that is, should one be asked to make some sloping lines rapidly, it would be expected from the preceding results, that he would move the hand in the direction of the radius of an angle of about 30° for strokes of abduction and of about 210° for strokes of adduction.

Vertical lines would tend to slope toward the right, rather than toward the left, when made by upward or abduction movements; that is, they would fall over into quadrant I, where the movements are more easily made.

Down strokes, or adduction movements would, if meant to be vertical, incline more toward quadrant III than quadrant IV, for the same reason. To test this point, the following experiments were made.

A vibrating bar or fork interrupted the current from a four-ampére battery, which was connected with the primary coil of a spark coil; the secondary coil was connected with a pen at one pole, and at the other pole with a writing board devised by Dr. Scripture This writing board consisted of a sheet of metal, *A* in Fig. 9, fifty-three and one-half by thirty-seven centimeters, set in a frame, with a board back; a binding post, *C*, at one corner, furnished the means of connecting the electric wire, *1*. A wooden pen holder, *D*, had a hole drilled lengthwise through the handle; a wire, *2*, running through this hole was soldered to the metal end which holds the pen. A small rubber tube,

as a means of insulation, enclosed the pen holder. A common writing pen could be inserted in the holder and removed at will. The wire connecting the pen holder with the spark coil was of sufficient length to permit of its resting over the shoulder as the subject was using the pen.

As the pen was placed near the writing board, sparks, caused by the interrupting of the current, passed from the pen to the board. Drum paper, heavily coated with smoke while on a drum, was then cut to the

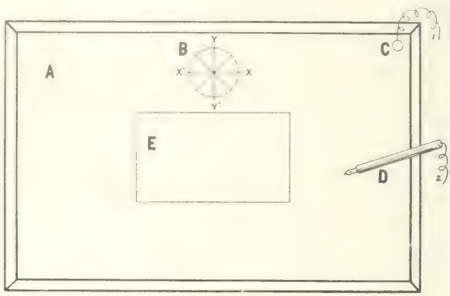

Fig. 9.

size fifteen centimeters by twenty-four centimeters and pasted, at the four corners only, to the writing board. The position of the paper is indicated by the small rectangle, *E*. As the pen was drawn rapidly over the paper, the sparks made dots that divided the line into equal intervals of time.

Fig. 10 shows a part of a record made by Dr. Scripture. A vibrating bar interrupted the current twenty-five times per second, so that the distances between the successive dots were passed over in one twenty-fifth of a second. This cut is reduced to one-fifth of the actual size of the record.

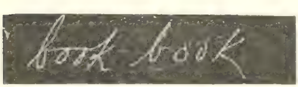

Fig. 10.

The subject in my experiments was seated before a table; the writing board with its lower edge resting on the table was supported by a heavy block in such a manner that it formed an angle of 30° with the table. A circle, *B*, divided by the *X* and *Y* axes into four quadrants, and with lines bisecting the quadrants, was drawn on the writing board, just above the position of the paper. A fork interrupted the current one hundred times per second.

The subject was requested to make five lines in the direction of each of

the radii as drawn on the circle. The length of the lines was not limited. A sample record, full size, with an arrowhead indicating the direction of the movement, is shown in Fig. 11.

Three of the subjects—*C*, of the first series of experiments; *D*, of the first and second series; and *X*—are graduate students in the university, and *G*, of the second and third series, a commercial college teacher. By

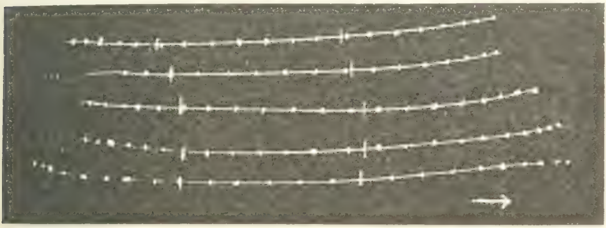

FIG. 11.

means of lines marked on the writing board, it was possible to place the paper in exactly the same position for all the subjects.

After the records were varnished and dried they were each fastened to a drawing board so as to be in the same relation to the edge of the board as they had been to the edge of the table.

The drawing edge of a T-square formed the base line for the protractor; and in this way the slope of the lines was easily and rapidly read off.

As the lines were not limited in length, the time of making the whole lines could not well be considered, but the distance of three centimeters was taken as nearly in the middle of the lines as could be estimated by the eye, and the time consumed in moving this distance was read. One-third this time was recorded as the time consumed in passing over one centimeter for the movement by which that line was made. As found by BINET and COURTIER[1] the greatest speed of a movement was at the middle of the line drawn. By taking the greatest speed for each line, a fair means of comparison was obtained. The lines varied in length from about four centimeters to nine centimeters. The shorter lines were made in quadrants II and IV, and the longest at the beginnings of quadrants I and III.

Table V shows the average time consumed in moving one centimeter

[1] BINET ET COURTIER, *Sur la vitesse des mouvements graphiques*, Revue Philosophique, 1893 I 664.

for each direction, by each subject ; also the average slope of the lines actually drawn for each slope intended.

TABLE V.

Time.

Subject	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	315°
C	28.6	32.2	44.4	57.6	25.2	27.4	37.6	45.8
D	15.2	11.[illegible]	14.4	20.2	13.2	14.6	16.8	15.6
X	21.2	30.2	32.6	53.4	21.4	36.6	39.4	43.2
G	18.6	24.8	30.4	43.2	17.8	24.2	33.6	36.0

Unit of measurement, $\sigma = 0.001^s$.

Slope.

[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	225	270	315
C	6.2	38.0	61.6	139.2	188.2	212.4	246.0	310.8
D	8.0	34.2	79.2	160.4	191.6	221.2	256.0	341.8
X	3.0	46.8	104.0	133.6	186.0	239.4	271.8	306.6
G	5.0	20.6	71.4	155.4	187.6	220.4	252.4	313.0

Unit of measurement, 1°.

The preceding tables showed that the movement at about 45° was more rapid than that at 0°.

In this table, for the second subject only, we find more rapidity in the angle 45° than for 0°. The slope of the disk is doubtless the cause of this change. As was found in the preceding tables, the slowest movement is about 135°.

The movements in the angle 225° for the preceding tables is more rapid than those of 180°, but in the case of every individual in this table, the opposite is true.

The tendency to incline the direction of the lines toward that quadrant or part of the quadrant where the easiest movements are made, is shown in the second part of Table V in the results of all the subjects except the third one. This record was obtained from the observer himself. He had noticed this tendency on the part of the other subjects, and a conscious effort to move more nearly in the exact direction indicated, resulted in the line being inclined in the opposite directions in the cases of the lines intended for 45°, 90°, 135°, 225°, 270° and 315°.

The lines for 0° and 180° show the same tendency as the records of the other subjects ; this, doubtless, is due to the fact that the observer did not notice any such tendency for these lines until he had begun to measure their slope.

B. Small movements.

First series of experiments.

The apparatus in the form used for the above experiments was not suitable for determining the speed of movements of smaller extent. In order to measure the time of movement for the distance of one centimeter for the angles of the four quadrants as in the preceding experiments, the following change in the apparatus was made.

The rod carrying the metal blocks, the revolving contacts and the wire frame used to raise the bamboo rods were removed from the desk.

A brass tube, *C* of Fig. 12, of three centimeters in outside diameter and twelve centimeters long, was suspended from the center of the upper

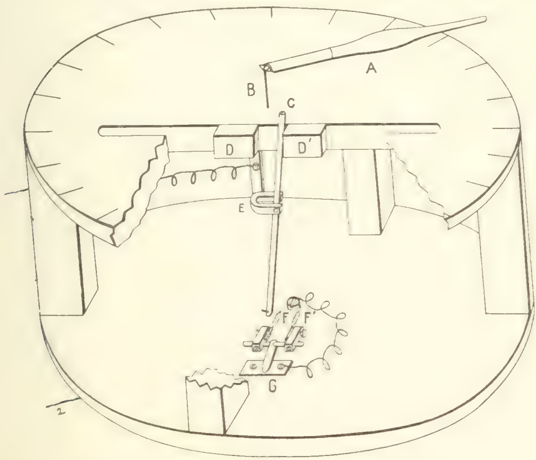

FIG. 12.

disc by an axle on a pivot, *E*, five and four-tenths centimeters from its top end, in such a manner that it might be caused to swing in the line of the slit only; the top end extended one-fourth of an inch above the top of the upper disc.

A T-shaped support, *G*, carrying adjustable contacts, *F* and *F′*, on the arms of the T was fastened to the lower disc, so that, as the tube suspended from the upper disc was made to swing, these contacts would be in the plane of the movement.

The contacts were so adjusted that from the time the lower end of the tube, in making one swing, touched the first, until it touched the second one, the top end of it had passed through the distance of one centimeter. As in the arrangement of the apparatus used before, the contacts were connected in parallel to each other, and in series with the DEPREZ marker.

The time of movement was registered on the drum for both directions of the swing of the tube. Stops, D and D', were placed in the slit of the upper disc so that the tube might swing just beyond the contacts. The entire distance through which the top end of the tube might swing was one and four-tenths centimeters. As the tube was caused to swing in one direction the time of the movement through one centimeter was recorded for that direction. As it was moved back the direction of the movement was 180° from the direction previously taken.

The subject was seated at the desk as before, with the exception that now the elbow rested on the desk. The desk was so adjusted, however, that the arms were close to the body, when the subject sat in an upright position.

In the place of the pen point, a wire, B, was loosely hinged to a common pen-holder. The subject held the pen-holder in any manner he chose. The wire upon it was permitted to slide into the tube; the end of the pen-holder rested lightly on the top of the tube.

With this arrangement it was possible to move the tube back and forth in the same manner as one might make lines with a pencil in the required directions.

The subject was requested to make movements forward and backward as rapidly as possible with the full arm, allowing the elbow to rest on the table and not making any movement of the joints of the fingers or wrist. The term, full arm movements with rest, will be used to denote these movements. Some of the subjects held the wrist flat, resting the arm on the cushion formed by the group of muscles just below the elbow. Most of them, however, took a more comfortable position of the wrist, allowing the palm to be turned toward the body, so that there was not so much of a cushion of muscle on which to rest the arm.

The forward movements were always in the direction of the radius for the angle used, while the backward movements were in the direction of the radius for the corresponding angle of the quadrant opposite.

At the signal "go" the subject made the first motion forward and back and continued to make them as rapidly as possible until signalled to stop. Records were made in this way for each angle marked on the disc as before.

Of the records for the angle 0°, the first, third, fifth and so forth, were

for the forward movements, the second, fourth, sixth and so forth, for the backward movements, or, for the angle 180°. So for each angle the odd numbered records gave the time for the movement in that direction, and the even numbered records the time for the movements 180° from this angle.

In reading ten records for each angle, the five odd numbered ones were placed in the column for that angle, and the five even numbered ones in the column for the angle 180° larger. The angles of the first two quadrants then produced five records for each angle of the four quadrants, and the angles of the last two quadrants furnished the other five records for each angle; thus ten records for each angle were obtained.

Table VI shows the averages of the results of nine subjects.

TABLE VI.

Subject	0°	15°	30°	45°	60°	75°	90°	105°	120°	135°	150°	165°
A	33.6	28.6	29.7	27.0	28.6	33.9	35.6	42.0	39.6	39.3	39.0	34.9
D	21.2	21.1	19.6	22.4	21.8	23.4	26.3	29.3	31.5	28.1	28.5	26.5
F	44.7	27.3	29.7	34.5	43.8	49.3	51.1	68.5	63.3	61.5	59.4	49.9
G	29.7	25.6	21.1	23.2	25.8	27.4	31.1	34.9	30.7	32.0	31.2	30.0
P	26.4	22.7	17.1	20.3	23.4	29.7	34.1	31.6	33.4	38.5	30.7	32.9
Q	35.0	30.4	30.0	32.8	36.1	35.8	38.3	42.3	41.4	47.3	42.8	38.1
R	32.7	24.5	23.9	25.8	28.5	31.8	33.9	32.8	34.0	35.3	32.7	29.9
V	23.6	22.8	27.5	29.3	33.6	29.9	38.8	37.7	41.4	41.3	42.5	35.3
W	32.2	28.2	24.5	25.2	26.8	28.4	32.0	33.4	35.2	41.4	38.3	33.6
Av.	31.0	25.7	24.8	26.7	29.8	32.2	35.7	39.2	38.9	40.5	37.6	39.5

Subject	180°	195°	210°	225°	240°	255°	270°	285°	300°	315°	330°	345°
A	31.5	28.4	28.9	30.4	33.9	34.8	40.1	41.9	38.6	39.7	38.4	34.2
D	21.6	21.3	19.6	19.5	24.4	27.1	30.1	29.2	30.6	30.4	26.2	25.6
F	39.1	28.2	30.8	32.2	42.7	45.7	47.0	58.4	56.2	57.0	55.3	51.0
G	29.6	23.6	20.5	20.7	27.1	29.2	33.3	37.4	35.4	36.6	34.3	33.1
P	29.2	22.0	17.1	20.4	23.0	30.2	34.9	32.1	31.2	35.7	35.5	37.7
Q	38.3	31.8	31.0	32.4	35.2	37.2	38.0	38.3	39.6	44.4	42.9	41.0
R	32.6	26.3	21.3	22.8	32.1	32.3	34.6	32.7	34.1	37.1	34.6	26.9
V	26.3	25.0	32.3	33.3	36.8	40.0	37.8	39.9	45.5	47.2	43.1	33.0
W	31.6	27.4	26.5	26.2	28.2	30.2	33.0	33.8	35.8	39.9	36.7	39.5
Av.	31.1	26.0	24.2	26.4	31.5	34.1	36.5	38.2	38.6	41.0	38.6	35.8

Average for quadrants I and II, 33.1; average for quadrants III and IV, 33.0 average for all quadrants, 33.1.

Unit of measurement, $\sigma = 0.001^s$.

The subjects were *A*, *D*, *P*, *Q*, *R*, *V*, graduate students, *F*, a mechanic, *G* a teacher of a commercial college, and *W*, a college freshman.

These movements are very nearly the same as those recorded in the two preceding tables, and the results resemble those somewhat. There

is, however, this difference: the movements for all angles of quadrants I and II average 33.1^σ, while those of quadrants III and IV average 33.0^σ. Without doubt, this very slight difference is due to chance. That is, movements towards the body have no advantage over those from the body when the freedom of the full arm movement is restrained by resting the

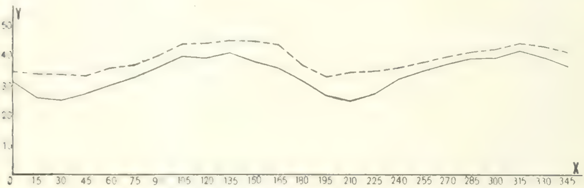

Fig. 13.

arm on the muscle below the elbow. The most rapid movements are, as before, found to be at the end of the first third of quadrants I and III.

The results in Table VI are shown graphically in Fig. 13. The curves are plotted in the same manner as those of Fig. 7. The ordinate shows the relative time consumed in the movement, the abscissa, the size of the angle. The solid line shows the results of Table VI.

Second series of experiments.

As originally planned, there were ten persons tested in the preceding experiments on full arm movements with rest. The same ten persons were then requested to make movements as before, but with the fingers and wrist, not allowing the full arm to move.

Records were obtained from nine of them, but the tenth one, the janitor of the laboratory, found his fingers too stiff to permit of ease of movement. Five trials were made upon him. He wished to make the movements rapidly, and in order to do so would change the position of the arm so much, that it seemed impossible to class his records with those obtained from the other subjects. In order that a comparison might be made between the full arm movements with rest, and the finger-wrist movements, the records of but the nine were given in Table VI.

The records for these nine persons in the finger-wrist movements are given in Table VII.

Fig. 13 presents these results graphically; for the sake of comparison, the results of Table VI are shown in the same figure. The dotted line in that figure shows the results of Table VII.

Table VII gives an average of 38.8^σ for the first two quadrants and

TABLE VII.

Subject	0°	15°	30°	45°	60°	75°	90°	105°	120°	135°	150°	165°
A	26.9	26.8	24.7	23.9	24.0	25.9	29.1	28.0	31.1	32.3	32.2	33.6
D	26.6	26.8	26.1	24.4	28.0	28.8	34.2	33.8	37.6	36.8	40.4	36.1
F	32.6	33.5	30.2	28.8	29.0	32.0	36.9	42.6	44.0	40.4	44.0	40.8
G	28.5	26.3	30.1	29.9	39.8	39.6	31.4	41.8	42.6	43.0	41.4	41.0
P	35.9	30.9	43.2	41.5	38.1	38.2	37.8	43.9	43.8	47.5	48.9	50.1
Q	44.2	37.2	37.2	33.8	36.6	37.7	41.1	47.8	48.5	51.0	47.6	44.5
R	28.2	31.5	26.5	31.3	36.9	39.1	46.6	48.6	46.4	52.4	51.2	43.1
V	41.4	38.6	32.9	31.1	36.2	35.3	43.3	45.1	46.9	45.6	44.6	44.4
W	45.0	51.3	50.7	52.7	52.1	53.0	57.2	57.3	51.1	50.8	48.4	54.9
Av.	34.4	33.7	33.5	33.0	35.6	36.5	39.7	43.3	43.6	44.4	44.3	43.2

Subject	180°	195°	210°	225°	240°	255°	270°	285°	300°	315°	330°	345°
A	24.8	23.9	25.7	23.8	24.9	24.8	25.7	25.4	26.7	28.1	27.8	28.3
D	26.9	27.4	26.1	24.9	30.9	31.6	35.3	37.8	36.5	39.2	39.6	38.3
F	36.9	33.2	32.0	26.8	31.0	32.5	32.9	35.2	39.3	39.1	38.6	35.8
G	26.7	25.9	29.4	38.5	47.6	40.1	47.8	48.5	45.4	44.0	42.0	41.6
P	35.1	32.5	42.7	46.8	35.6	40.4	42.8	41.6	52.6	52.6	50.1	46.6
Q	45.1	32.5	31.1	30.7	30.6	34.0	39.0	45.1	44.9	50.1	42.3	43.3
R	27.6	32.1	25.5	29.1	34.1	47.5	38.2	40.5	42.5	43.6	39.0	38.3
V	44.9	40.5	37.8	30.4	29.7	26.3	32.8	36.3	39.7	43.2	41.1	41.5
W	56.7	43.5	53.1	56.4	52.0	56.3	55.7	54.2	49.0	50.8	50.7	53.0
Av.	36.1	32.4	33.7	34.2	35.2	37.1	38.9	40.5	41.8	43.4	42.4	40.7

Average for quadrants I and II, 38.8; average for quadrants III and IV, 38.0; average for all quadrants, 38.4.

Unit of measurement, $\sigma = 0.001^s$.

38.$^\sigma$ for the last two. This is a greater difference than was found in Table VI, but, as in all the tables, excepting the first one, that difference is in favor of movements toward the body.

The full arm movements with rest average for all the angles 33.1$^\sigma$; the finger-wrist movements average for all the angles 38.4$^\sigma$; hence, there is greater ease in the full arm movements with rest. The difference is 5.3$^\sigma$, that is, the finger-wrist movements are 16% more difficult,—or, at least, require 16% more time than the full arm movements with rest.

The slowest movement with the fingers and wrist required 44.4$^\sigma$ [135°]; the quickest 32.4$^\sigma$ [210°]; for the full arm movements with rest, the same angles required 40.5$^\sigma$ and 24.2$^\sigma$. This shows a loss of 3.9$^\sigma$ and 8.2$^\sigma$ respectively in the finger and wrist movements, or of 9.6% and 33.8% respectively. It will be noticed that the movements compared here are those for corresponding angles. Should a comparison be made between the vertical strokes of the finger-wrist movements, and the movements at about 30° of the full arm movements with rest, as WOOD-

WORTH[1] has done, we would find the advantage in favor of the full arm movements with rest to be 14.9$^{\sigma}$, or, 60.1% of the time required for the full arm movements with rest. This is greater than he found it to be.

Fig. 14 shows the results of Tables VI and VII plotted after the manner of Fig. 8. The dotted line represents the time for the finger-wrist

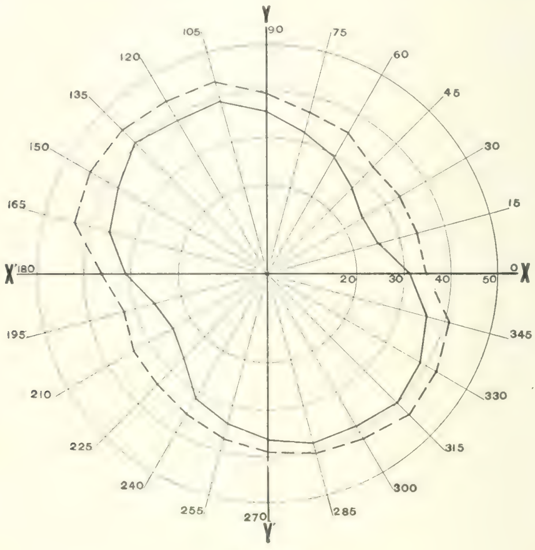

FIG. 14.

movements, the solid line that for the full arm movements with rest. The distances along the radii show the relative time consumed in the movements.

II. THE NATURAL SLANT OF THE OVAL.

Since it is impossible to draw a circumference of a circle by one sweep of the hand or arm, the figure formed must be an oval, and one of its diameters longer than the others.

Has the longest diameter of ovals, made in the attempt to draw a circle, a definite direction for each individual, or, does the direction vary so that no definite angle can be expected?

[1] WOODWORTH, *The accuracy of voluntary movement*, Psychological Review. Monograph Supplement, 1899, III, 109.

The style of writing is due very largely to training. Should the ovals of persons trained to write, especially of adults, be examined, we would expect to find the slant of the oval to be the same as that of the principal strokes in their writing.

If some particular slant rather than another is natural to man, it seems that it should be observed in the ovals made by the untrained.

Do children who have never been taught to write, but who know what a ring is, when trying to make a ring, make an oval whose "slant" is 90°, or is the slant more, or is it less than 90°?

Through the kindness of Dr. Stewart H. Rowe, Principal of the Lovell District in New Haven Public Schools, it was possible to obtain some ovals from thirty-nine children in the Lovell kindergarten.

These children had not been taught to write, but had used the pencil and brush for drawing. They have been permitted to hold the pencil as they pleased, no special form being taught them. The brush is held much higher than the pencil, but aside from being told to hold the brush nearer the top, there were no instructions in regard to the manner or holding it.

The children were seated at tables eight feet long and sixteen inches wide; the tables differed in height; the lowest was nineteen and one-half inches, the highest twenty-one inches; the tops were divided by lines parallel to the sides of the table into one inch squares. The chairs used were from ten and one-half inches to twelve and one-half inches high. The children were seated as comfortably as it was possible to seat them with such chairs and tables. The smaller children were placed at the lower tables.

The tables did not permit an ideal position in writing. Some of the elbows, when lying on the tables, were much too high for ease in writing. Usually in such cases the child would stand up or rest his weight on one knee on the chair in order to get a more comfortable position for making the movements.

There were two classes in the school; one present during the morning session, and one in the afternoon session. The morning session began at nine o'clock and closed at fifteen minutes after eleven.

The classes were each divided into two sections. Each section was seated in a different part of the hall used for the school. Three tables arranged in the form of a quadrangle, with the chairs outside of the quadrangle and the teacher's table within, made it possible for the children to all face the teacher and be quite near her, as she stood by the table.

The teacher of the second section was following out her regular program

while the teacher of the first section was directing her pupils in the work to be done for the experiment.

A lead pencil and a sheet of manilla paper of the size nine and one-half by eleven and one-fourth inches were placed on the table before each child in the first section. The paper was placed in front of the middle of the body, one of the short sides being on the first line of the table.

The teacher then said: "Now children, this man has come to see us to-day and he wants us to make some little rings on the paper. We must be sure to keep the edge of the paper on the first line in front of us. We must not move the paper at all; we must keep that real still; and now we will make some rings for him, about as big as a penny, as big as my ring"—showing the children her ring.

The children were carefully watched and if there was any tendency to move the paper, a weight was placed on it at the upper left hand corner. If this did not prevent its moving, the teacher or the experimenter would hold it with the hand.

After at least ten rings had been made by each, the papers were marked with the name of the child who had made the ovals upon it, and with signs to indicate the form of movement used, and collected, and other papers of the same quality and size distributed; these papers had each a ring of 1.4 centimeters stamped upon it near the top. The line forming the ring was five-tenths of a millimeter in width.

The children were again cautioned to keep their papers on the lines and to hold them still; then they were asked to make some rings like the rings on the paper.

After a number of rings had been made by each child—using the same precautions as before to see that the paper had been held in one position while making them—the papers were marked as before and collected.

A third piece of paper like the first one, a brush and a small glass containing a water color were then placed before each child; the children were now asked to make small rings with the brush; the size, "as big as a penny" was asked for.

After marking and collecting these papers, a fourth paper was given them like the second, with the ring stamped on it near the top, and they were now asked to make rings with the brush just like the ring on the paper.

These papers were marked as before; and, after collecting them, this section of the class was kept busy with the regular work, and ovals were made by the second section in the same manner.

While the first section were making the ovals, the second section had sung and played with building blocks. No work had been done with a pencil or brush.

The work of making the ovals was begun at fifteen minutes after nine and completed at fifteen minutes after eleven; as this was the time for the session to close, the pupils were then dismissed.

The session for the afternoon class was from half after one to fifteen minutes after three. The entire time was required to get the ovals. The class was divided into two sections as before, and the ovals were obtained in the same way as at the morning session.

There were twenty present in the morning class. One of this class refused to do as requested, so his papers were not used. The papers of two of the twenty-two children present in the afternoon were not used for the same reason.

The thirty-nine papers used were gone over carefully and lines drawn through the ovals in the direction of the longest diameter. The judgment by the eye was sufficient to determine the longest diameter in most of the ovals; in a very few cases the difference was so slight that it was deemed best to apply a graduated scale.

The observer had taught geometry for five years; his familiarity with geometrical figures was such that illusions of length produced in him by intersecting lines or the figures ordinarily used to demonstrate such illusions were very small as compared with the average person. This qualification, perhaps, made him better suited than the average person for doing this sort of work.

The papers were each fastened to a drawing board, square with the board and in the same relative position as to the observer as they had been to the child.

The drawing edge of a T-square determined the base line for a protractor, and the angles of the lines of the longest diameters, previously drawn, were in this way rapidly measured. The angles were measured according to the convention mentioned above (p. 21), the right hand portion of the X axis being considered as the initial position, and the radius moving counter clockwise.

Some of the papers contained only ten ovals and some many more. Two papers contained as many as thirty-nine ovals each. As it was impossible to make a selection of the ovals, in every case all the ovals were measured on each paper.

The average for each paper is shown in Table VIII.

The first and third sets, A and C, of the ovals were considered as having been made with freedom. Certainly it would be impossible to make the children understand what was wanted of them in any manner that would put less constraint upon them. Limiting the size to about a penny, or the size of the finger ring, was necessary to prevent making them so

large that it would be impossible to get a sufficient number of ovals on the paper. In most cases the size was nearer that of a silver half dollar or three centimeters.

TABLE VIII.

	Pencil.				*Brush.*			
	A.		B.		C.		D.	
Subject	*Av.*	*P.E.*	*Av.*	*P.E.*	*Av.*	*P.E.*	*Av.*	*P.E.*
1	80.4	33.3	116.9	25.5	72.7	25.7	70.3	16.1
2	83.8	26.5	95.8	15.9	111.9	28.2	68.3	18.2
3	71.6	25.7	73.1	17.2	88.4	23.6	99.7	19.3
4	38.9	26.4	57.8	28.3	71.3	41.9	38.5	27.6
5	107.3	20.6	99.7	31.4	76.4	18.6	72.9	24.6
6	62.3	21.2	76.2	17.8	82.8	29.6	67.8	28.0
7	68.4	18.0	101.4	39.2	86.1	30.0	79.9	21.9
8	54.9	18.5	73.1	33.8	51.5	20.1	53.3	23.9
9	67.3	14.4	41.6	5.7	60.0	20.4	66.0	29.5
10	85.1	22.5	84.1	27.8	90.7	25.5	80.5	22.7
11	101.6	27.2	83.8	21.3	51.2	14.2	65.8	23.3
12	107.0	13.2	91.0	13.1	82.0	41.2	63.8	49.4
13	114.8	25.4	110.2	19.5	51.3	23.9	64.8	26.6
14	60.4	31.1	80.1	21.2	78.9	41.7	116.3	36.6
15	88.0	16.2	85.5	24.8	68.7	8.9	65.5	25.8
16	79.8	24.5	59.4	15.9	71.4	18.9	70.9	17.3
17	94.3	23.8	94.6	17.3	94.5	30.1	79.1	21.0
18	84.0	11.3	80.2	16.5	72.3	32.8	65.0	19.2
19	96.5	30.3	104.6	18.6	78.3	30.3	75.4	20.1
20	82.1	21.6	79.1	33.2	101.1	23.0	85.9	20.8
21	81.0	24.8	89.3	26 4	76.1	34.0	80.8	39.0
22	82.8	35.3	87.8	29.4	50.1	16.0	53.0	14.8
23	98.7	7.4	108.0	17.1	56.4	32.6	66.5	36.8
24	94.0	33.7	76.8	43.0	132.4	27.8	92.9	38.2
25	44.4	5.9	63.6	15.2	78.9	33.3	88.5	30.8
26	83.3	22.5	91.1	17.8	97.4	34.2	72.8	41.3
27	84.5	18.3	87.1	20.1	65.5	30.0	84.0	24.6
28	67.1	29.6	42.8	18.4	78.8	35.8	99.9	32.5
29	111.8	18.6	70.5	30.6	110.5	27.7	72.0	28.0
30	43.9	24.2	79.4	20.2	103.6	31.1	94.7	29.1
31	112.5	29.3	87.7	29.3	62.7	39.3	68.9	41.8
32	100.9	19.1	82.1	12.8	52.9	29.5	49.4	26.0
33	105.8	14.1	89.1	17.3	74.5	20.5	87.7	13.4
34	89.5	28.1	86.4	25.6	103.0	32.7	59.8	13.2
35	71.9	14.5	66.7	26.5	48.2	25.5	43.1	10.0
36	41.5	25.6	71.4	31.0	57.6	27.8	79.6	32.1
37	90.7	22.6	96.9	28.9	46.2	17.1	86.7	22.3
38	94.8	28.8	89.3	28.4	70.7	34.1	79.8	22.1
39	85.7	36.3	39.6	14.0	72.8	25.3	70.9	24.2
Av.	82.4	22.9	82.1	23.0	76.4	27.8	73.8	6.0

Av., average of each paper; *P. E.*, immediate probable error (p. 25). Unit of measurement, 1°.

The pencil or brush was held by the child in any way he wished. Some changed their manner of holding it often. Three children began by holding the pencil by the ape grasp, that is, the thumb not opposed to the fingers; afterwards they made use of a manner much like that of the average person except that the fingers were too cramped for ease. In two cases the pencil was placed between the first and second finger. Most of the children imitated the manner in which the teacher held her pencil, that is,—the pencil was held between the thumb and second finger, with the forefinger resting upon the upper right side of the pencil, and the pencil resting against the knuckle of the forefinger.

They grasped the pencil more firmly than was necessary, but approached very nearly to this position.

The second and fourth sets, B and D, were made under the partial constraint of having a copy to reproduce. The copy had very little influence on most of the children, the size of the ovals made being about the same as before. In some cases there was an earnest endeavor to reproduce the copy, at least the size of it; but in most cases there was no difference in the appearance of the papers as to there being more uniformity in the size or the slant of the oval.

The slant of the oval in some papers varied from 0° to 175°. In most cases, there was much less variety of slant.

A free full arm movement was used in most cases, the exceptions being: numbers 1, 3, 5, 8, 10, 12 and 20 in exercise A, 1, 14 and 37 in exercise B, and 30 in exercise D, in which the finger and wrist movement was used entirely; and in numbers 15, 17, 21 and 29 of exercise A, 3, 7, 8, 10, 14, 15, 17, 18, 21, 24, 29, 35 and 37 of exercise B, 24 of exercise C, and 21 and 28 of exercise D, in which the finger and wrist movement was used part of the time, and the free arm movement part of the time.

The ovals made by the finger and wrist movements were smaller than the pattern, the others much larger.

The pencil does not permit so much freedom of movement as the brush. We might expect the brush then to give a slant that is more natural.

If the experiments whose results are given in Tables I to VI inclusive, have shown the direction in which the movement is easiest, we should expect the greatest length of the oval to lie in this direction.

The averages for the four sets of ovals are 82.4°, 82.1°, 76.4° and 73.8° respectively. The slight difference between the averages of the first and second sets is to be accounted for wholly by chance. The difference between the third and fourth sets may be accounted for in the same way. The difference between the first two sets and last two sets,

however, may be due to some cause other than chance. This cause may be found in the difference between the instruments used in making the ovals, and the results seem to warrant this conclusion.

The Tables I to VI inclusive show that a straight line may be made at a slant of between 30° and 45° with the most ease; the line perpendicular to this line being the most difficult. In making the ring the intention is to make all axes of the same length and the hand is consciously turned out of its initial direction; but, it does not move easily in the direction of the moving radius for the angles from 120° to 135° and the corresponding angles from 300° to 315°.

The movement is made with one continuous swing of the pencil or brush. The eye has no time to correct the movement,[1] the muscle sense alone governs the extent of movement in either direction.

As the feeling of effort made by the eye in running along vertical lines is greater than that made by running over horizontal distances of the same length, and so results in an over estimation of the vertical distances, so, we find that the greater effort required to make movements in the direction of the radii of quadrants II and IV causes the distances passed over in those directions to be overestimated by the muscle sense.

After the ring has been made, the eye discovers the error due to the muscle sense. A desire to correct this may cause an extra effort to be made in the direction in which the movements require a greater amount of muscular strain, and so the angle made by the longest axis of the oval may be increased ninety degrees in the second attempt. The attempt to correct the difference causes the variety of slants as found on some papers.

Let the reader with his eyes open make a "ring" rapidly. Then closing the eyes, but with no conscious effort to correct the length of the diameters, that is, moving the pencil with the same purpose as before, make another one; the similarity of the two ovals will be striking. There will be no effort to correct the diameters of the ovals if several rings are made with the eyes closed.

It would be possible to get children to make rings with the eyes closed or, perhaps better, blindfolded; but the fluctuating back and forth of the longer axis in the method employed with the children in this experiment, will doubtless on an average produce very nearly the same slant as that which would be obtained with the eyes closed.

III. The movement to be used in writing.

The following discussion is based partly on the preceding experiments and partly on more general principles of psychology and facts of experience:

[1] Woodworth, *The accuracy of voluntary movement*, Psychological Review, Monograph Supplement, 1899 III 72.

Children, in first learning to write, use the finger and wrist movements. Such movements by the small hands produce very small letters, and the teacher must give a great deal of attention to the movement of the full arm in order to train the child to make use of the movements which permit the formation of larger letters.

The full arm movement when the elbow is resting on the desk, for which we have used the term full arm movement with rest, is much more rapid than the finger and wrist movement.

One of the subjects, *G*, in some of the experiments recorded here, uses the forearm movement, suggested by WOODWORTH,[1] for writing his signature—that is, the side to side swing of the forearm—"the direction of the line of writing is nearly toward the body, or more exactly in line with the forearm. A backward movement of the whole arm carries the hand along the line, while the side to side motion of the wrist and forearm make the separate strokes." The top of the paper, for this form of movement, must be inclined to the right. This movement produces writing with very sharp angles.

It is certainly an easy movement but the head will be inclined toward the right to make the reading easier for the oblique position of the paper. The straight middle position for the paper would cause the elbow to be pushed outward and forward, so that the forearm may be parallel with the bottom edge of the paper; this encourages a stooping of the shoulders, and necessitates bending the head forward so that the chin is pressed closely to the throat, compressing the air passages. Such a position cannot be used in the schools, and we question the value of using it even for adults.

The finger and wrist movement permits round forms for the letters, and so a more legible hand; it is very much slower than the full arm movement with rest, requiring not less than 16 per cent. (p. 45) more time than those movements, so that the loss of time in producing the writing doubtless balances the gain in legibility.

Experience has shown the teachers of penmanship that neither movement, by itself, should be used to the exclusion of the other: if used in combination, the freedom of the forearm can be united with the more delicate touch and shaping power of the fingers, enabling the writer to execute easily and rapidly, with less fatigue than with either movement separately.

For small children, the greatest attention should be given to developing a good, full arm movement. Even with much care in this direction

[1] WOODWORTH, *The accuracy of voluntary movements*, Psychological Review, Monograph Supplement, 1899 III 106.

the fingers will be used largely. This means of course that the elbow is to rest upon the desk. The child is unable to properly coördinate the movements of the arm. If the arm does not rest upon the desk, it will be held tightly to the side of the body in order to aid in the control of the movement. After the arm has been well trained, the rest will often be considered not necessary. The trunk of the body should be inclined a little forward, the back straight, the upper arms hanging nearly vertical; the breast not touching the front edge of the desk. If the desk is sufficiently low, this permits an easy position for writing. The head, inclined slightly forward, should not be brought too close to the writing. The left forearm should be so placed that it will make an angle of about 60° with the right forearm. All the larger movements should be made with the full arm, also all of the strokes directly up and down. The fingers should aid in forming the turns, producing thereby broader turns than the full arm movements alone would tend to produce.

The preliminary training of the child should be to give it perfect control of the hand. Clay modeling in the kindergarten is available for this purpose. This should be accompanied by the use of the brush. Most children have the slate or lead pencil placed in their hands at the start. The slate or lead pencil requires a firm grip and some pressure in order to produce friction enough to make the path of the point visible. The habit thus formed of gripping the pencil is seldom eradicated.

The narrow path of the pencil permits small figures, the wrist or edge of the palm near the little finger rests upon the table, and the hand is moved by short, limping steps along the line.

The broader path of the brush makes a small figure or character impossible for the small hands, and a large full arm movement is easily acquired. By the continued use of the brush a higher degree of muscular sensitiveness is gained; the child soon learns to make finer and more regular lines. The bright colors and solid figures produced by the brush are of much more interest to the child than the empty outlines produced by a pencil. No attempt should be made to form letters until the child has acquired a fair degree of control of the movements of the arm. This should be followed by producing large letters with the brush, care being taken to see that the forms of the letters are properly impressed upon the child.

The use of the pen will follow naturally from this. The brush does not require a hard grip, and the pen will be held lightly. Soft pens and light penholders should be used. Attention should be given to the manner of holding the pen; the wrist or side of the palm must not rest upon the table; the third and fourth fingers should support the hand. The ink used should be a heavy black or dark blue, and the paper a light yellow.

The slate has become a matter of history at least so far as our schools are concerned. The lead pencil will always be with us, but the child should be furnished with very soft ones.

The earlier training with the brush and pen will have taught the child how to prevent rubbing the soft pencil mark.

IV. The best slope for writing.

The question of economy in time always enters into the discussion of any system of writing. The experiments recorded above have shown the directions in which the movements may be most easily made. They all show that the most rapid movements are near the end of the first thirds of quadrants I and III.

By means of experiments similar to those made by Woodworth, it has been recognized that the most rapid short movements are those made by swinging the forearm back and forth, resting on the muscles below the elbow as a pivot.

The exact angle at which the movements must be made with reference to the body had not been determined previous to the experiments recorded above, but the direction was well enough determined to cause the exponents of the sloping systems of writing to affirm that a slope of 48° permitted the most rapid writing. In order to understand fully the direction of the principal strokes for this slope, we must consider the directions given to students of these systems.[1]

Four positions of the writer may be properly assumed: the left, the right, the right oblique and the front; but since the demands of school hygiene are that the front position only should be used, we will consider that position only. For such a position the students are instructed to sit directly in front of the desk, keeping both sides equally distant from it. The paper should be turned so that the bottom edge forms an angle of 20° with the edge of the desk.

If a slope of 48° be added to this angle we find that the down strokes, that is, the strokes determining the slant, are 68° from the X axis or the front line of the body; or, to use the notation employed above, these strokes are along the radius for the angle 248°, and lie in the latter part of the third quadrant.

The slope recommended for general writing and for instruction in the schools was 52°; this in like manner would mean an angle of 72° to the X axis or 252° for the down stroke.

The up strokes of the letters are made at an angle of 34° to the base line: because of the slope of the paper their direction is that of the radius

[1] Spencer, Spencerian Key to Practical Penmanship, 27, New York, 1872.

for the angle 54°. Exceptions to this rule are found in the case of the letters [illegible] and [illegible], the initial stroke of which would be at an angle of 39° to the base line; and of *v*, whose up strokes should be at 45°. So we find that the up strokes varied from 54° to 65° from the X axis.

The slant that resulted in great beauty was 60°. Then the range of slant varied from 68° to 80° with the X axis, according as speed or beauty was most required; the down strokes therefore lay in quadrant III between the radii for the angles 248° and 260°.

The experiments on large movements, p. 37, have shown the great tendency to change the slope toward the directions most easily made. The hand in trying to use a slope between 60° to 48° would unconsciously take a greater slope of from about 50° to 38°. The lines lying at such small angles to the base line become hard to distinguish. The distances between the lines are very much diminished as the slant from the vertical is increased, and the turns, being more and more narrowed, come to be angles. Such angular writing is very hard to read rapidly.

The hand acquires a slope in writing that is usually farther removed rom the perpendicular than the model used as a copy in learning to write. This has been observed by teachers of writing.

By the time the habit had been acquired, and the individual's style developed, it was found that the slope actually used was a much greater deviation from the vertical. The vertical systems, which gained general acceptance so quickly, form an excellent means of producing a legible and rapid hand. The child is taught to make his strokes vertical; the hand acquires the habit, and the deviation from this direction is not usually more than 10°.

The paper before the writer, being square with the desk and immediately in front of him, has the same position with reference to him as the disc in the desk used for the experiments on small movements, p. 41. The down strokes then take the directions from 255° to 270°. The commercial colleges noted the deviation from the vertical, and, not knowing the cause, concluded that this direction was "natural" to the hand and they accordingly adopted the slant of about 70°. We recognize that with the paper in the straight middle position, the position recommended for vertical writing, the movements for this slant are made in the same directions as those insisted upon in the SPENCERIAN systems of writing.

As the results of our experiments would indicate, it was found that the slope taught was not adhered to.

Such a style of writing is not as legible as the copy set before the pupil, nor as the style used by the students of the vertical systems.

Gradually the commercial college men changed their copy till they now use a system very nearly vertical; the angle which the principal strokes make with the X axis is about 85°; the paper is inclined to the left 10°, thus a slope of 75° with the base line is produced. The deviation from this slope which we now expect results in a slope of about 70°, for the individual styles of the students. The habit has been acquired by the student and the writing produced is very legible.

The public schools are no longer upon insisting the strictly vertical forms for every pupil.

Objection is made to the position of the paper before the middle of the body. For a paper five and one-half inches wide, the left side should be placed directly in front of the middle of the body, according to many instructors.

The Supervisor of Penmanship in the public schools of a city where one of the vertical systems is used, suggests that for older pupils, and to aid in rapid writing, the lower left hand corner of the paper should be moved even farther to the right—perhaps an inch and a half—and the top of the paper then inclined toward the left about ten degrees. Down strokes that approach 270° are then made for the principal ones; this means that the slant taught would be one of about 80° with the base line, and we are not surprised to find that, when the individual's style is acquired, it proves to be a slope of about 75° or that of the copy used by the business-college men.

Because of the construction of the arm, the movements near the end of the first third of quadrants I and III are most rapid and the attempt to make a line of any given slope will result in a slope which approaches nearer the easiest movement for that quadrant except where a conscious effort is made to prevent this; in writing, no such effort is made. A copy of considerable slope, though legible, will produce a style of writing with a greater slope and less legibility; it is in this that the vertical systems have a decided advantage over the older, or SPENCERIAN systems.

A very near approach to the copy is of course desirable, but is not attained by most writers, and never in any case without long practice. Because of the tendency to increase the slant from the vertical, children when learning to write in the SPENCERIAN systems are unable to produce results that can be easily read. With the so-called vertical systems, an increase in the slope, though found, is not sufficient to render the writing illegible, and a readable hand is more easily acquired. Care in instruction will tend to improve the legibility and the final style for the student is much better than that of most of us who were instructed in the SPENCERIAN systems.

The question of the legibility of the styles acquired by the individuals must decide which slope is the best. The slope desired must be chosen first, and the copy made a few degrees nearer the vertical.

The greater the slant from the *Y* axis, the more rapid will be the writing, even to the extent of causing the principal strokes to make an angle of 45° or less with the *X* axis.

Such a direction for the strokes would require that the top of the paper be turned somewhat toward the right, in order that the resulting writing may have the necessary slope with the base line to make it legible.

The strictly vertical slopes are not so easy to make. The slant of about 75° permits legible writing; as the slant approaches the vertical, it is of course more legible, but as the angle decreases below seventy the legibility decreases rapidly. The down strokes of such a slope are in the direction of the radius for about 255°, provided the paper be square with the desk. The up strokes are about 15° less than this slope or about 60°.

Vertical writing would require 270° movements for the down strokes and, when the upstrokes do not coincide with the down strokes, movements of about 75° for the up strokes.

The experiments on small movements, p. 41, show that the movements for 270° require only 7 per cent. more time than the movements for 255°: the 75° movements require about the same proportional increase of time more than 60° movements require.

Most of the systems of writing have this difference of slant between the up and down strokes. The turns are those of a rather narrow ellipse. This is not as legible as a more circular or round turn. With broad round turns the up strokes may follow the lines of the down strokes and thus produce a style of writing more nearly like the printed characters. This is desirable, especially for the child making his first efforts.

The elliptical slanting up strokes, as our experiments show, are more rapid than the vertical straight strokes, but speed is not a consideration for the beginner. For the young child, the purpose is to fix upon him the visual and muscular sense images of the different letters and to properly associate them.

When the letters such as *m*, *n*, *w*, *u*, *t*, and so forth have the up and down stroke coincident as far as possible, the appearance of the letters is much more simple, and the form more easily fixed in the child's mind.

The reason for turning the paper about 10° toward the left is that in moving the hand along the base line, or in writing a long word, the inclination of the paper admits of an easier movement.

The time required to move the hand along the 10° radius is 20 per cent. less than the time for a movement along the radius of 0°.

Any turning of the top of the paper toward the left will cause the child to turn his head toward the left, so as to make the line joining the eyes parallel to the base line of the writing.

Many lecture halls have small tablets secured to the right arm of the chairs in order that the student may have a support for his notebook. During the present college year we have given attention to the positions taken by the students while writing. The tablets are almost wholly to the right of the writer; the notebooks are so placed that their tops are turned towards the left when on the tablets. There was not one student observed at any time during the year whose body was in an upright position and head square on the shoulders while writing his notes. The head was inclined towards the left and the spine curved, in order to twist the line joining the pupils of the eyes into a position parallel with the base line of the writing and as nearly directly above the paper as possible.

When the notebooks rested on the knees the same bending of the head was to be seen.

This is the old habit acquired at the school desk during the writing lesson. For the children who are just beginning to write, the question of speed is of no importance. The fundamental thing is to educate the muscles that the proper coördinations may be made to form legible characters. Let every precaution be taken to keep the position of the child upright, the head square upon the shoulders. A front straight position, with the left side of the paper on the median line or a little to the left of it, is the only one that admits of this. Let the up strokes be merely a vertical retracing of the down strokes when the form of the letter will permit.

A base line is desirable that the eye may more surely guide the hand in writing a line across the page, or in properly lining the letters in a word. Other lines on the paper will cause the child to give more attention to the spacing and height of the letters than to the form. Much energy will be devoted to causing the letters to just touch the top line; the head will be drawn down to the paper, the back curved and the shoulders bent forward in the attempt to see just beyond the point of the pen, so that it may not be moved too far in the upward stroke.

Connecting the letters is desirable; for, as the child slowly moves the hand in producing the up stroke, the eye has time to estimate the distance, and the tops of the letters are for that reason very nearly in a line. Without these guiding lines, the tool used in writing is raised from the

paper at the base line; the movement toward the place from which to begin the formation of the next letter is rapid; the muscles not trained, overestimate the distance, and the top of the next letter is often half the height of the letter above where it should be. The child may be governed by the size of the copy and so make this letter entirely above the line; or, if much has been said about writing on the line, the letter may be made enough larger than the copy to permit of its resting on the line. The next jump may be underestimated in the endeavor to not go too high, and, to make the letter full size it must extend below the base line; perhaps influenced by the strict injunction to write on the line, the letter is reduced in size to suit the space. Such results are discouraging to the child. With connecting lines, the movements are made more slowly, and the distances estimated.

As skill is acquired, the pupil may see that these lines are not essential parts of the letters and may omit them altogether; but this will not happen until speed has been acquired, and the need of reducing the time of forming the letters results in dropping all unnecessary lines.

Binet and Courtier,[1] found that separated letters may be made more rapidly than connected ones.

It is a great strain on the nerves of the hand and arm to attempt to keep a constant pressure on the paper for all strokes. Raising the pen or pencil from the paper removes this strain; the upward movements may be made much more freely and with greater ease if it is not necessary to trace the path of the movements on the paper.

The up strokes at the beginning and end of a word, which the small child must make with so much care in the older systems of writing, are of no assistance to the reader, if they do not actually hinder him.

For rapid reading, only the lines essential to the forms of the letters should be on the paper.

Jackson states[2] that "Continuity in writing is one of the preëminent elements of speed." He admits later: "It cannot be too strongly impressed upon our teachers that the laws and rules which determine shape, size, direction and junction of strokes and letters are not fixed and immutable, but arbitrary and conventional;" but even this cannot prevent him from expressing astonishment that a certain person, one who fails to connect his letters according to the Jacksonian method, "a voluminous writer and author," and whose correspondence is "immense," should "still survive in remarkably good health."

[1] Binet et Courtier, *Sur la vitesse des mouvements graphique*, Revue Philosophique, 1893 I 667.

[2] Jackson, Theory and Practice of Handwriting, 51, London, 1893.

The statement that "every word should be finished before removing the pen" is often printed in bold type in books on writing and insisted upon by most teachers of writing.

An examination of the writing of these persons, however, will show that they do not follow this principle. One such person in a letter to the secretary of a publishing house upon the subject of writing stated: "The fact is—as perhaps this rapid scrawl of mine will in a measure evince—the joining of letters is the one efficient means of securing rapidity and continuity in the composition of a word. Of course, however, the manner of joining, leading doubtless to some modifications of the system now in vogue, needs to be determined and reduced to a practical art." In this section of the letter, dealing with the question of rapidity, we may see spaces between the letters in twenty-eight cases.

The places within the words where the pen has been raised and a letter has not been joined to the preceding one are indicated by italicising the letter after each such jump of the pen, in the following manner: "The fact is—as perh*a*ps this rapid s*c*rawl of mine will in a me*a*sure evin*c*e—the joining of letters is the one effi*c*ient means of securing rapi*d*ity and continuity in the comp*o*sition of a w*or**d*. Of course, however, the manner of joinin*g*, le*ad*in*g* doubt*l*ess to some mo*d*ific*a*tions of the syst*e*m now in vo*g*ue, nee*d*s to be determi*ned* and re*d*u*c*e*d* to a pra*c*ti*ca*l art." In these twenty-eight cases there can be no doubt at all that the pen was raised while writing the words, for the letters are not joined. The manner of joining, in six cases, is such as to show that, though the lines actually meet, the pen had been raised.

A specimen of the writing of the principal of a business college, of this city, was examined. In this as in practically all business colleges, the students are taught that a continuous motion is necessary for speed. The specimen examined, however, showed many letters disconnected. The teacher though using a continuous motion, when possible made that motion easier by removing the friction of the pen upon the paper, and relieving the muscle of the hand for an instant from maintaining the constant pressure upon the paper.

The shading alternating with fine hair lines contained in some of the older "advanced" copy books provided a means of relieving this strain by varying the pressure. The hair lines were always the less important ones. Many rapid writers make few lines with the upward stroke of the pen. These movements as our experiments have shown, are harder to make than the down strokes.

If the hand is trained well to move the proper distance the line of

the writing is not badly broken by a failure to estimate the distance in the rapid upward jump of the pen; nor is the forward movement in passing from one letter to the next, overestimated to such an extent as to destroy the continuity of the word. This training we believe can be more easily acquired by using connecting lines with beginners.

For the beginner, we would suggest that: the copy contain all the connecting strokes; that in the letters *m*, *n*, *u* and similar forms the up strokes coincide with the down strokes as far as possible, producing broad round turns; that the slope be 90°; that the paper be placed straight before the child, the left edge on or a little to the left of the median line.

If the up strokes slope off from the down strokes in the letters mentioned, the legibility is not so great and the illusion produced by the copy is that the down strokes slope backward. Such a copy might cause the child to write a back hand.

The copy may show the lines connecting the letters without a break in a word, but we should not insist that the pen be not raised during the writing of the word; it doubtless will be raised, but the downward strokes will meet the connecting lines, and the child will be aided in estimating the height of the letters and the spaces between them.

The position suggested for the paper permits the child to follow the pen more easily with the eye.

With such a manner of writing children will acquire a legible hand quickly. The great mass of the people who leave the schools early in the course will be able to write a legible hand.

There should be no studied effort to disconnect the letters. Such an effort may cause a full stop at the close of each letter. If the hand has been trained to a continuous movement, when writing rapidly without any effort to raise the pen, it will be seen to jump over the distance in the upward strokes when the down strokes must retrace the path made by the up strokes. This manner of writing soon leads to the omission of many of the connecting lines. The result is more legible than before these lines were comitted, and the speed is increased by the saving of time in the rapid free movement of the pen.

The sizes of the letters should be reduced considerably from the large copy placed before the beginners. Each pupil should be allowed to make those sizes naturally agreeable to him, after the forms of the letters have been thoroughly mastered. The space between the lines should be sufficient to prevent any appearance of crowding, and to permit paying no attention to the lines.

The lines are often a means of retarding the speed, for some attention is given to keeping on them. If the hand has been trained to write on a

line, it will not be a difficult task to continue in a straight line without the aid of the ruling.

When speed is demanded from the advanced pupil, a slight slanting of the paper to the left may be suggested. The movement in forming the letters is now a fixed habit, and their direction with reference to the body will not change. The result is that a slant between 75° and 85° is now used; this is legible and permits greater speed than the strictly vertical.

The question is often asked: Is there any valid objection to teaching a back hand? The experiments recorded above show conclusively that there is at least one valid objection, that is, such a slant requires movements that are comparatively very hard to make and so reduces the speed to such an extent that it must be considered impracticable.

For the students of stenography the question of speed is of the greatest importance. The older systems contain many characters that must be made by movements in the direction of the radii of quadrant IV. These movements are very slow, requiring 27 per cent. more time on the average than the movements of quadrant III and 24 per cent. more than those of quadrant I.

One system of shorthand, introduced into this country about 1893, contains no characters that must be made in either quadrant II or IV, and the slope of the characters is such that they lie very near the middle of the quadrants I and III. In this respect, then, this system of shorthand is the most rapid yet devised. Since shorthand never requires a series of lines parallel to each other to be closely connected, this slope does not reduce the legibility.

RESEARCHES IN CROSS-EDUCATION

(Second Series)

BY

WALTER W. DAVIS.

Further experiments on the effects of practice in voluntary movements have been made since the publication of my first paper on the subject.[1]

I. Transference of practice effects.

Experiments with the maximum grip of the hand were begun in March, 1900. An oval spring dynamometer of the usual form was employed. The dynamometer test is not one of endurance, but of strength. To make a good record requires a strong impulse for only a moment of time.

Fifty subjects were secured—25 men and 25 women, all students or instructors at Iowa College, Grinnell, Iowa. The maximum pressure that each could exert on the dynamometer was determined for both right and left hands. To avoid the variation due to fatigue, the records were taken in the order R, L, L, R, and then an average taken of R, R, and of L, L. These average records were called the initial records for right and left hands. After the initial records were secured, practice was begun and extended over a period of three weeks, the subject exercising his grip four times per week. At the end of the practice final records were taken in precisely the same manner as at the initial test. The difference between the two records gave the amount of gain.

Great care was observed to have all the conditions of the initial and final tests precisely the same. Four points were carefully watched. 1. The dynamometer was placed in the hand face downward. This prevented the subject from seeing his own record, and there was no danger of the fingers stopping the progress of the pointer. 2. The instrument was carefully placed so that the pressure was exerted in a line perpendicular to the long axis. 3. One side of the dynamometer was placed in the crease of the second joint of the fingers, so that when it was gripped it pressed against the second row of phalanges. 4. Care was exercised to prevent the subject from pressing the hand or arm against the body.

The initial and final tests and the daily practice occurred at the same time of the day and as nearly as possible under exactly the same condi-

[1] DAVIS, *Researches in cross-education*, Stud. Yale Psych. Lab., 1898 VI 6.

tions. The practice consisted in gripping the dynamometer 10 times on each occasion, with intervals of 2 or 3 seconds of rest after each grip.

The subjects were divided into five groups. These groups with the method and manner of practice are shown in Table I.

TABLE I.

Kind of practice by the subjects.

Subjects.	Method of practice.	Imstrument used.	Hand practiced.
Group I.	Vigorous.	Dynamometer.	Right.
Group II.	Light.	Dynamometer.	Right.
Group III.	Light.	Dynamometer.	Left.
Group IV.	Light.	Cylindrical stick.	Right.
Group V.	Light.	Dynamometer.	Right and left.

TABLE II.

Characteristics of the Subjects: Men.

Serial number.	Age.	Group. (See Table I.)	Previous training.	Temperament.
1	27	I		nervous.
2	23	II		phlegmatic.
3	23	III	track athletics.	phlegmatic.
4	22	IV	track athletics.	motor.
5	22	V	baseball.	motor.
6	22	I		phlegmatic.
7	22	II		nervous.
7½	22	III	track athletics.	phlegmatic.
8	21	IV		motor-phlegmatic.
9	20	V	general athletics.	phlegmatic.
10	20	I		motor.
11	20	II	general athletics.	phlegmatic.
12	19	III	track athletics.	nervous.
13	19	IV		phlegmatic.
14	19	V		nervous.
15	19	I		phlegmatic.
16	18	II		phlegmatic.
17	18	III		phlegmatic.
18	18	IV		nervous.
19	18	V		motor.
20	18	I		phlegmatic.
21	18	II		nervous.
22	21	III	track athletics.	nervous.
23	27	V	general athletics.	motor.
24	19	II	track athletics.	motor.
25	24	IV	lifting weights.	nervous.

Those subjects who practiced vigorously made 10 efforts daily, using the maximum strength at every effort. Those who practiced lightly

made 10 efforts using from about $\frac{2}{3}$ to $\frac{1}{3}$ of the maximum strength. The cylindrical stick was 3 inches long by 1 inch in diameter. Those subjects who practiced with both right and left hands made 5 efforts with each hand on each day.

In order that the influence of age might not affect the results sought, the subjects were distributed by fives among the groups as shown in Tables II. and III. By such a distribution the average ages of all the groups were nearly the same.

TABLE III.

Characteristics of the subjects: Women.

Serial number.	Age.	Group. (See Table I.)	Previous training.	Temperament.
26	25	I	1 year.	nervous-motor.
27	22	II	4 years.	phlegmatic.
28	22	III	2 "	nervous-motor.
29	21	IV	2 "	motor-phlegmatic.
30	21	V	1 year.	moderately phlegmatic.
31	20	I	2 years.	phlegmatic.
32	20	I	2 "	nervous.
33	20	III	1 year.	moderately phlegmatic.
34	20	IV	1 "	nervous-motor.
35	20	V	3 years.	nervous-motor.
36	19	I	2 "	motor-phlegmatic.
37	19	II	2 "	nervous.
38	19	III	1 year.	motor.
39	19	IV	2 years.	phlegmatic.
40	18	V	2 "	nervous-motor.
41	19	I	1 year.	nervous.
42	19	II	1 "	motor-phlegmatic.
43	18	III	4 years.	motor.
44	19	IV	1 year.	nervous-motor.
45	17	V	3 years.	motor.
46	17	I	1 year.	phlegmatic.
47	25	V	7 years.	motor.
48		II		nervous-motor.
49	21	III	2 years.	nervous.
50	24	IV	2 "	phlegmatic.

The previous training for the men and women was determined differently in the two cases. In Table III. the length of the period of physical training was reckoned from the number of years each subject had spent in the gymnasium in actual required class work, such work being compulsory except for seniors in the college. Since gymnasium work had not been required heretofore of men, it was necessary to set a different standard for them. This standard was proficiency in athletic sports.

The factors of the grip that affect the strength of the pressure on the dynamometer are very complex even in one individual. Factors which favorably affect the pressure in a certain person may be entirely counteracted by factors which affect the pressure unfavorably. In another subject the factors may oppose each other in an entirely different fashion. When many conditions that are favorable to the strength of pressure are present in one person we then expect that person to make a good record of strength.

For convenience we have classified all factors as either, (*a*) those that are not affected by a short period of practice such as that during which our investigation was carried on, or (*b*) those that may be affected by such practice. The first four factors belong to the former group, the others to the latter one.

1. *Effect of length of hand.*—This factor is of some importance. The longer the hand the longer must be the levers on which the muscles pull and consequently the greater the strength that may be exerted on the dynamometer. This factor may be almost entirely counteracted by others, as is evidenced in the case of the men in Table IV. The importance of this factor is shown quite clearly in the case of the women.

TABLE IV.

Length of hands as a factor in dynamometric pressure.

	Men:		Women:	
	Shortest hands.	Longest hands.	Shortest hands.	Longest hands.
Average length in mm.	186.3	200.1	166.8	180.2
Average pressure in kg.	48.1	48.2	26.0	30.3

To construct Table IV. the hands of all the subjects were measured with a measuring stick; the distance was measured from the primary crease on the front of the wrist to the end of the middle finger. The result 186.3^{mm} represents the average length of the 10 shortest hands among the men, 200.1^{mm} the average length of the 10 longest, all of the subjects being taken into account in one or the other class. In the case of the men, those who had short hands seem to have possessed other qualities that counteracted the disadvantage of the short hand. There was an altogether different arrangement of favorable and unfavorable factors in the case of the women. Of course, the number of measurements taken was too small to make any very important generalizations.

2. *Effect of length of forearm.*—MILLER[1] has found a definite relation between the length and the strength of the forearm.

[1] MILLER, *Relations of length and strength in the forearm*, Pub. of the Univ. of Penn., 1900 IV 153.

3. *Effect of muscular development of the hand and arm.*—However important the part that this factor plays in the amount of the pressure, it is not probable that it was affected by the short practice of this experiment. It has been pretty clearly demonstrated too in this investigation that the test under consideration is not nearly so much one of muscular power as it is one of mental power.

4. *Effect of temperament.*—GALEN'S universally adopted classification of the temperaments into choleric, melancholic, sanguine and phlegmatic was not followed. For several reasons a different classification was more suitable to our needs. Since it was our purpose to discover the readiness of the subject to respond to practice (considering exercise as a stimulus, and development of motor ability as the response to this stimulus), our needs were subserved better by a classification based fundamentally on the ability to respond quickly to a given stimulus, and not by one based on both the strength and quickness of the response. An outline of such a classification is presented as follows:

weak / weak

Nervous—medium—Motor—medium—Phlegmatic.

strong / strong

The nervous temperament reacts quickly to a stimulus; this reaction may be either weak, medium or strong. The phlegmatic temperament reacts slowly; this reaction may be either weak, medium or strong. The motor temperament stands midway between the nervous and phlegmatic, if we consider time of reaction as the chief element in classification. The motor subject's reaction may be either weak, medium or strong. This classification makes strength of reaction a secondary consideration and quickness of reaction the primary consideration.

There is a question whether the nervous person does not as a matter of fact react more weakly than the phlegmatic one. To throw some light on this point, the average grip was determined for all the men classified as phlegmatic. The same was done with the records of the motor and nervous subjects. The average results are given in Table V.

TABLE V.

Relation of strength of grip to temperament.

	Number of subjects.	Temperament.	Aver. grip in kg.
Men.	10	phlegmatic.	49.7
	6	motor.	49.6
	6	nervous.	43.3
Women.	7	phlegmatic.	30.3
	14	motor.	28.9
	4	nervous.	22.5

The table shows that the phlegmatic subjects in this particular group of individuals are slightly stronger than the motor subjects and considerably stronger than the nervous subjects. The fact is especially noticeable among the women. The fact noted, however, does not prove that the phlegmatic temperament *per se* is stronger than the nervous or the motor; nor would it be even though the results obtained here should be substantiated by similar ones secured from a large number of observations. There are several factors that tend to counteract, in the nervous type, any inherent power that it might have for the exertion of great strength. These are the mechanical factors that we have enumerated above.

In the first place the nervous person is smaller, generally speaking, than the phlegmatic person. This fact accounts in a measure for his being nervous; for the whole mass of muscles to be controlled being so much smaller, innervation is accomplished much more quickly. The hands are smaller and shorter, the forearm shorter, and hence the subject is placed at considerable disadvantage. Moreover, this disadvantage increases in more than arithmetical proportion, as the result of one factor at least, namely, the shortness of the hand. When the dynamometer just fits the hand and when the muscles and levers work in just the right arrangement, a good record is expected. But when the hand is so small that the pull must be made with the first phalanges rather than with the second, a tremendous disadvantage is met with, and there is a much smaller record.

This theory is strengthened by a comparison of the length of hand for men and women with their respective dynamometric records. Table VI. shows this comparison. While in length the women's hands averaged 89.7% that of the men's their strength was only 58.7% as great. The difference in strength would not be so great if it were not that the leverage of the small hand is not so good.

TABLE VI.

Relative strength of men and women.

Subjects.	Aver. length of hand in mm.	Aver. pressure in kg.
Men.	193.2	48.2
Women.	173.4	28.3

Above a certain point however great length of hand is of no advantage. Reference to Table IV. makes this statement clear. The men averaging 200.1^{mm} for length of hand were very little stronger than those averaging 186.3^{mm}. The very large hand is at a disadvantage because the dynamometer is pressed on one side, now, by the third phalanges

instead of the second. Table V., too, supports this conclusion. The natural inference is that the best method of grasping the dynamometer is to place one side in the crease of the second joint so that pressure is made with the second phalanges.

It seems necessary, then, in discussing temperament, to consider all the factors enumerated above, and the problem becomes more and more complex. An effort will be made in Part II. to present a somewhat more comprehensive view of the influence of temperament on bodily strength and its relations to muscular development.

Of the factors influencing the strength of dynamometric pressure we have named four: (1) length of hand, (2) length of forearm, (3) muscular development of the hand and arm and (4) temperament. These four factors are not easily affected by a short period of practice.

5. *Coördination.*—By coördination we mean the ability to innervate a particular muscle or set of muscles so specifically that very little of the motor impulse escapes into muscles not concerned in the movement that is being performed. It is the ability to direct motor impulse; or speaking from another standpoint it is muscular control. The term is best expressed in common speech by the word "knack." This factor is much improved by practice. Athletes possess it in a greater degree than men who have not had special physical training.

6. *Stored energy.*—This must be found in two places, in the motor nerve cell and in the muscle. It must be present in the nerve cell to generate the motor impulse and in the muscle to cause the contraction. Its absence in either place would make muscular action impossible. Fatigue means simply a loss of potential energy—hence fatigue is inability to do work. The authorities are agreed that complete loss of potential energy is sustained sooner by the central nerve cells than by the peripheral organs. Hence we say that central fatigue appears before peripheral fatigue and so a protection to the muscles is afforded. Potential energy must be present in large amounts in tests requiring a maximum exertion through a long period of time.

7. *Will power or volitional power.*—Potential or reserve energy is of no use until it is changed to kinetic energy. This change is effected in all primarily conscious tests by conscious volition. One's ability to accomplish feats of strength depends, in great measure, on his power to transform the potential energy of the motor cells into the kinetic energy of the nervous impulse. In addition to this, the strength of a muscle must depend on the readiness with which the potential energy of the muscle is changed by the motor impulse into the kinetic energy of the muscular contraction.

Lombard, in his experiments with the ergograph, has shown that in tests of endurance the potential energy must fail before the will tires. In tests of strength which require the quick accumulation of the potential energy of the nerve cells and its ready change into the kinetic energy of the nervous impulse, it is the power of volition that fails. The conversion is not readily made. Still it is probable that even in tests of strength an abundance of potential energy is favorable to great exertion.

8. *Impressibility.*—The fourth factor that is affected by practice is the openness of pathways for nervous impulse. By disuse of any nerve or set of nerves, the pathway of the impulse is obstructed, and innervation of the muscle is made difficult.

Factors 5, 6, 7 and 8 are all affected by practice and exercise. Volitional power increases by exercise. Exercise stimulates the growth of nerve and muscle cells and influences the storing up of energy. Coördination improves by practice. The frequent passing of a nervous impulse keeps the necessary paths free and open.

To get a basis for comparison Table VII. was arranged to show the average transference of the effects of practice. With the averages shown there the transference of any group may be compared. Only groups I., II. and III. were put into the table. Group IV. was left out because the conditions under which the subjects of this group worked, were not, as will appear later, at all favorable to transference. Group V. also very evidently should not appear in the table.

After casting out Groups IV. and V., and certain individuals for various reasons, the results for 12 men and 14 women are shown. The results in the columns headed *First final* were secured immediately after the three weeks' practice; those under *Second final* four days thereafter, no practice occurring in the meantime. The numbers under the heading *Direct gain* represent the gain made by the hand practiced: those under *Transference* the gain of the hand not practiced. In Groups I. and II. the transference is from right hand to left; in Group III. it is from left hand to right.

Two facts show themselves clearly from a glance at the table:

(*a*) *The results due to fatigue.*—As a result of the practice the men gain, at the first final, 8.06% with the hand exercised, and 9.92% with the hand not exercised. At the second final after a rest these figures are reversed; 9.53% and 8.81%. That is, the hand practiced, made, at the first final, a smaller percentage of gain than did the hand not practiced, but at the second final test its percentage of gain was the greater. In the case of the hand exercised it seems that the fatigue of a three weeks' practice had caused its low record at the first final; but at the second

final it had reached its normal tone by reason of a four days' rest. The other hand had at the second final already lost some of its gain. Hence its lower record.

TABLE VII.

Percentages of gain by practice.

I. Men.

No.	Group.	First final test. (After three weeks' practice.) Direct gain.	Transference.	Second final test. (After four days' rest.) Direct gain.	Transference.
1	I	0.48 %	7.05 %	2.88 %	12.94 %
2	II	12.22	11.34	13.33	13.53
3	III	8.41	14.68	10.28	18.81
7	II	12.05	10.35	15.06	00.57
7½	III	10.20	9.74	4.90	— 2.96
10	I	3.67	9.42	2.29	11.00
11	II	12.30	26.77	10.53	17.70
12	III	3.45	17.11	5.52	12.50
16	II	— 0.47	1.12	1.89	— 0.56
17	III	— 2.08	0.00	3.12	— 4.02
20	I	20.10	2.73	25.92	18.03
24	II	17.39	7.74	18.63	8.22
Average		8.06	9.92	9.53	8.81

II. Women.

No.	Group.	Direct gain.	Transference.	Direct gain.	Transference.
26	I	12.50	0.00	7.69	— 6.05
28	III	6.31	— 2.56	10.52	— 0.85
31	I	9.00	2.90	12.82	5.07
32	I	— 3.06	— 3.33	3.82	4.61
33	III	21.21	15.69	6.89	5.21
36	I	1.42	— 4.91	10.00	— 4.10
37	II	20.00	43.94	18.88	33.33
38	III	2.63	— 0.77	12.28	13.95
41	I	5.51	— 3.53	— 1.04	1.17
42	II	0.78	7.14	9.37	7.93
43	III	10.68	9.73	25.38	24.71
46	I	— 3.09	1.06	3.09	— 2.12
48	II	1.61	3.03	4.83	6.06
49	III	5.21	9.80	6.25	6.86
Average		6.45	5.84	10.05	6.81

The effects of fatigue are seen, too, in the women's averages. Here, the hand not exercised seems to have been affected by the *fatigue* of the other, which it is quite evident was very great: and it does not reach its normal tone until after a short rest. There is a point in time shortly after the end of the practice when the effects of fatigue have just vanished.

This point would of course differ in all individuals. If a test could be made just at this time it would show the best record that the hand practiced could possibly make. Such a record would be better than that of the unpracticed hand at its most favorable time. Still in a test of strength like that with the dynamometer this best possible gain would undoubtedly be nearly the same for both hands.

(*b*) *The difference between the results secured from men and women.*

A comparison of the averages for men and women shows: (1) that the women gave more negative results; *i. e.*, there were more failures among them to make gains from practice, there being five negative results registered for the men and eleven for the women; (2) that there is a greater regularity in the gains made by the men. These are only two examples among many observations made throughout the research, indicating greater reliability, in tests of motor ability, among men than among women. Boys in their abundant out-of-door life gain a more perfect and thorough muscular control than it is possible for girls to do. This is in accord with the results obtained by GILBERT in his experiments on voluntary motor ability, on reaction-time and on the judgments of weights.[1]

After these few words in general it will now be necessary to study the results in connection with our classification of the individuals into groups.

Table VIII. shows the average relative gain of each group for both men and women. The groups will be considered separately.

TABLE VIII.

Comparison of group gains.

Groups.		First final. Direct gain.	First final. Transference.	Second final. Direct gain.	Second final. Transference.
I.	Men.	12.68%	7.01%	10.36%	14.32%
	Women.	3.62	—1.30	6.06	—0.20
II.	Men.	10.69	9.46	11.88	7.89
	Women.	7.46	18.03	23.71	15.77
III.	Men.	5.88	11.18	5.95	6.08
	Women.	9.21	6.38	17.26	9.77
IV.	Men.	1.00	—2.63	——	——
	Women.	2.90	—3.38	5.55	1.51
V.	Men.	1.70	6.03	3.88	9.27
	Women.	3.78	8.69	9.31	13.91

Group I.: *Vigorous practice with dynamometer, right hand.*—This group is the only one that was practiced vigorously. A curious fact that can not be fully explained at present, is shown by a comparison of the results from the records of the men and women. There is a decided transfer-

[1] GILBERT, *Researches on the mental and physical development of school children*, Stud. Yale Psych. Lab., 1894 II 61, 64, 66, 78.

ence shown by the men but more by the women. It is rather startling to note that the women did not gain with the hand not practiced. Of the six women in this group, three at the first final test gave minus results with left hand, one, zero, and two, small gains. It should also be remarked, however, that even for the hand practiced two women failed to make any gain. An improvement is also noted in the second final test, where the right gained 6.06% and the negative result of the left was reduced from 1.3% to 0.2%, indicating that the effects of fatigue had begun to pass away before the second final test was made. The temperament of women as contrasted with that of men may be suggested as a probable explanation of the facts noted. The more nervous temperament of the women was strongly affected and seriously fatigued by the vigorous practice. It may be said that the two women of this group who made gains with the left hands at the first final were classified as phlegmatic. This class of people, since they react slowly to stimulus of any kind, would not be so strongly affected by the heavy practice as persons of a more nervous temperament. This consideration will be further discussed in the section on temperament.

Group II.: *Light practice with dynamometer, right hand.* The subjects of this group show, in general, a greater gain than those in Group I. This is especially true of the women. Our conclusions in regard to the fatigue shown by Group I. are corroborated here. Especially clearly, in the case of both the men and the women, is the fact shown that fatigue causes the hand practiced to make a smaller record at the first final test than the unpracticed hand can make. And for both men and women there is a reversal of the figures at the final test: the unused hand having already begun to lose the effects of practice.

Group III.: *Light practice with dynamometer, left hand.*—Three observations are made from an examination of the results secured by the practice of this group of subjects.

1. Practice of the left hand increases the strength of the right as certainly as practice of the right increases the left. This has already been shown by Woodworth. In reality it would seem that the transference may be greater in right-handed persons from left to right than from right to left. This supposition is supported in a measure by the next two observations made from the results given by this group of subjects.

2. The men gained more with the right hand than with the left. Women gained more with the left.

3. There is not a reversal of these figures in the second final test as was noted in Groups I. and II. That is, the effects of fatigue are not so manifest.

It is probable that these facts are explained by certain theories in regard to the conduction of the motor impulse. The impulse is more apt to follow old paths. In new paths greater resistance is met, and the impulse may be retarded or deflected. Reference is made in this connection to WISSLER'S conclusions on the diffusion of the motor impulse. Now the right hand is used much more than the left, though this statement is not so true of girls as of boys. Hence there would be a greater diffusion of the impulse into the right arm through the use of the left, than would happen *vice versa*. So there would be a greater transference in right-handed persons, from left to right than from right to left. This conclusion is strengthened by the fact that there is no reversal of the figures in the second final test. Fatigue did not appear in so great measure as in Group II., who practiced in precisely the same manner as Group III., except that they exercised the right hand instead of the left. There was greater diffusion of the motor impulse in Group III., hence less fatigue resulted.

Group IV.: *Light practice with cylindrical stick, right hand.*—Two noteworthy observations were made in regard to this group.

1. The gains in development for the hand practiced were small. The average gain for the men was only ½ kg. (1%); for the women, 0.85 kg. (2.9%). These gains are significant when compared with the gains made by the first three groups. They are much smaller than the gains made by the subjects of investigation by ANDERSON,[1] who found a gain of 6.3 lbs. (= 2.9 kg.) for right hand and 7.8 lbs. (= 3.5 kg.) for left, the right hand alone being practiced. There was, however, a distinct difference in the methods of practice followed in the two investigations. In one case the practice was made vigorous, in the other light. With the subjects in Group IV. the exercise was probably not favorable to the development of will power; in Anderson's investigations the conditions were favorable for such development, the subjects being instructed in the daily practice to exert all their strength. Further comparison of these results is made on page 78.

2. There was an absence of any transference of the effects of practice to the unused side. There is an actual loss recorded for the left arm. The men lose, at the first final test, 2.6%, the women lose 3.3%.

Observations (1) and (2) would seem to indicate: (*a*) the great importance of becoming used, in the daily practice, to the instrument with which the tests are made. The muscles learned how to contract properly for pressure of the cylindrical stick but gained no advantage

[1] ANDERSON, *Study in the effects of physical training*, Amer. Phys. Education Review, 1899 IV 3.

from this knowledge for gripping the dynamometer: (*b*) the importance of volitional power and coördination in the transference of the effects of practice.

Group V.: *Light practice with dynamometer, both hands.*—In this group both hands together received as much practice as only one hand in the other groups: *i. e.*, each hand made 5 pressures of the dynamometer in each daily practice. As much work was done in practice as was done by Groups II., III. and IV., but it was distributed between the two hands.

The following observations are made from an examination of Group V.'s practice and tests:

1. The tendency of both men and women was to refer the effects of practice to the left hand (see Table VIII.). The left hand being naturally weaker and less expert than the right there is a greater chance for development in its case.

2. The effects of fatigue are shown more clearly here than in any other group: *i. e.*, there is a greater ratio of difference between the gains of first and second final tests. The ratio of first final gain to second final gain is represented in Table IX. There is a great regularity of the ratios comparing the results from men and women subjects, much greater than in any other group. This shows that the effects of direct exercise are more regular than the transference effects.

TABLE IX.

Ratio of gains in first and second final tests.

	R.	L.
Men.	1 : 2.22	1 : 1.53
Women.	1 : 2.43	1 : 1.60

It is now proposed to attempt the determination of those factors of practice that are influential in causing the transference of practice effects. We do not mean to say that "cross-education" will be explained simply by showing the factors necessary for the transference. The deeper physiological and psychological explanation can be made only after a complete understanding is had of volition, of voluntary movement and of the coördination of mind and muscle in the production of movement. It can be pointed out, however, from results secured by several methods of practice that the development of certain factors in neuro-muscular activity are of the greatest significance in bringing about the education, both of the parts practiced and also of other parts which seemingly are not directly concerned.

The methods of practice referred to in this connection are in part in-

volved in the groups discussed above; in addition, an investigation by Dr. Anderson, of Yale University Gymnasium, will be freely discussed. Dr. Anderson's experiments were made in the winter of '97–'98, his subjects being secured from one of his classes. I was a member of this class and one of the subjects of the experiment. Hence it is not overstepping the bounds of logical procedure to compare intimately, results secured by two observers at different times and places.

Dr. Anderson's subjects practiced by two methods. One group, A, practiced vigorously with a cylinder of wood, 3 × 1 in.; the other group, B, practiced vigorously with the dynamometer. The only difference then between the two methods of practice was in the use of differently shaped instruments. For comparison, the methods of practice of Dr. Anderson's subjects and the results secured from their practice are arranged in a table with the methods and results of practice of the subjects of Groups II. and IV. Table X. shows this comparison together with the factors that seemed to contribute most largely to such results. In all the groups

TABLE X.

The factors of transference.

Experimenter.	Group.	Instrument.	Method.	R.	L.	Factors.
Anderson	A	Cylinder.	Vigorously.	6.3 lbs.	7.8 lbs.	Volitional power.
	B	Dynamometer.	Vigorously.	11.7	13.2	Volition. Coördination.
Davis	IV	Cylinder.	Lightly.	1.1	—2.6	Volition.
	II	Dynamometer.	Lightly.	10.6	11.5	Coördination. Volition.

only the right hand was used in practice. The average gains for all groups are given in pounds.

We should judge *a priori*: (1) that there would be a disadvantage in practicing with the cylinder; and that subjects using it would not gain as much as those using the dynamometer. We say in common speech that there is always a knack to be learned in acts that involve strength or skill; (2) that those practicing lightly would not gain as much as those practicing vigorously. Our second *a priori* judgment is not supported by very decisive results.

It is not necessary to compare each group of the table with all the others. We may compare Groups A and B to determine the effect on the results of practice, of using differently shaped instruments when the practice is vigorous; and Groups IV. and II. to determine the results when the practice is light. That an intimate comparison of the four groups might be made is proven by an examination of the results of

practice of Group B, Table X., and Group I., Table VIII. The method of practice is precisely the same in the two groups, but the time of practice varies. In spite of the difference in the time of practice the gains made are very nearly the same in both cases. The average of first and second final for Group I. is R, 11.95 lbs.; L, 10.45. This is even less than the gain made by ANDERSON's Group B though the practice of Group I. was longer. This indicates that the volitional power necessary for a certain act probably reaches its maximum development with only a short practice.

Table X. shows that the results secured vary in a remarkable degree. Group B practicing vigorously with the dynamometer makes the greatest gain; R, 11.7 lbs., L, 13.2 lbs. Group IV. practicing lightly with the cylindrical stick gains the least; R, 1.1 lbs., L,—2.6 lbs. An examination of the conditions of practice surrounding each group will explain, in a great measure, the reasons for such a wide variation of results.

It seems quite evident that vigorous practice calls for the exercise in a large degree of volitional effort. Light practice does not call for such exercise. It would seem quite as probable, too, that practice with a cylindrical stick could not cultivate a nice coördination of mind and muscle that would be efficacious in a test with the dynamometer. That is, the learning of the "knack" for one instrument will not help us out much in the use of another. So we are led to conclude from an examination of the striking results of Table X. that the variation of these results is due chiefly to the development in different proportions of two important factors of practice, both of which factors are very necessary and very evident in tests of this kind. These factors are: (1) *volitional power* or *will power;* in connection with this factor *attention* may be named: (2) *coördination;* or in terms of common speech, "*knack.*"

1. *Volition.*—The dynamometric pressure is emphatically a test of will power; a test of the subject's ability to send down to the hand and forearm at an instant of time an immense impulse for muscular contraction. The muscles involved can probably make use of all the stimulus that can possibly be sent down to them.[1] Will power and attention—for our purpose the terms may be considered as equivalent[2]—are factors that may be easily and quickly developed by practice.

2. *Coördinative ability or knack.*—The second factor is apparently of nearly as much importance in producing gains by practice. By knack we mean the proper coördination of mind and muscle in the performance

[1] Mosso, *Ueber die Gesetze der Ermüdung*, Arch. f. Anat. u. Physiol., Physiol. Abth., 1890, 89.

[2] James, *Psychology*, II 568, New York 1890.

of a muscular act, the ability to properly adjust one's muscles to the test at hand. It was noticed that the subjects gained much more, with the hand practiced, during the first part of the test than during the last part. This was as true of those who practiced lightly as of those who practiced vigorously. Both of these factors, then, were developed early in the test. This ability to properly govern the muscles involved is learned the more quickly, the brighter and more apt the subject is.

3. While we do not eliminate the storing of potential energy in the motor nerve cells, from among the factors affecting transference, yet it seems that in a test like this one it is of somewhat minor importance. The test is not one of endurance, but of strength. It is admitted though that the experiments carried out here have hardly been complete enough to determine this point. It may be, in fact it is quite probable that it is so, that the development of volitional power and of potential energy go along together and that it is quite impossible to separate and distinguish them by an experiment like the above. To determine this point further experiment is necessary, differing from ours both in method and kind.

Much less could the transference be caused by the growth of muscular tissue, the storing of potential energy in the unused arm. The method of practice was such that muscular tissue could hardly be developed, certainly to no extent appreciable by any means of measurement at hand at present. Only ten pressures of the dynamometer were allowed daily, requiring less than one-half a minute for the whole process, hence the probability of any development of muscular tissue was reduced to the minimum. Even in the dumb-bell test,[1] which was an excellent one for the development of muscular tissue, the girth gains for the left arm were small and the tissue developed did not prove efficacious for lifting the weight.

It seems then that there are in this test two factors the development of which are chiefly instrumental in causing a gain in the hand practiced —volitional power and coördinative ability. These are both of them factors that one would judge might, when developed, affect the unpracticed side as much as the side practiced. An examination of Table X. will show when each factor is preëminently present.

In Group A, learning the knack of gripping the stick would be of no advantage to the subject because the tests for a record were made with an entirely differently shaped instrument. There could be no gain made in the ability to coördinate mind and muscle in the accomplishment of this special test. But there had been an exercise of effort. Every day

[1] DAVIS, *Researches in cross-education*, Stud. Yale Psych. Lab., 1898 VI 23.

the will had been exerted to its utmost for ten successive pressures. The gain made by this group must be attributed to the development of volitional power. This gain was shared in by the left hand, unpracticed, the amount in its favor being due to the effects of fatigue on the right hand after three weeks of practice.

It is apparent that both factors were present in Group B. The gain of the left hand, unpracticed, was 13.2 lbs., 69% greater than its gain in Group A. Theoretically this 69% may represent the influence of coördination in causing a gain of strength, compared with 100% as representing the gain due to the development of volition.

The subjects of Group IV. made practically no development with either hand. There was a slight gain with the right and a loss with the left. This was true of the women too, though the left did gain slightly at the second final test. The method of practice seems altogether unfavorable to the development, in any effective sense, of either volition or coördination. It is probable that volition is developed in a limited degree but that the gain due to this development is counteracted by what may be termed a loss of coördination. The subject having become accustomed to the shape of one instrument has learned to use certain muscles after a certain order or fashion. At the final test, using a differently shaped instrument but with the same object in view, the tendency may be to use the same muscles in the same series or order as in the practice with the stick. The result is a lack of such coördination as will be effective for a good test with the dynamometer. One set of muscles obeying a mistaken impulse behind it, gets in the way of the set that should respond. The loss of effectiveness in one factor counteracts the gain in the other.

For the same reasons there would not be a great development of volition in Group II. Coördination was probably responsible for the greater part of the gain. The gain is not so large as that of Group B, where both factors work at their best and in harmony with each other.

The results and observations seem to point to the conclusion that practice with the dynamometer develops two factors chiefly—*volition* and *coördination*. The development of these factors cause a gain in strength of each hand whether practiced or unpracticed. Both factors are among the faculties that we call mental.

It is not asserted that other factors may not run parallel with these. In fact it seems probable that the storing up of energy in the motor cells of the brain is a necessary concomitant with the development of volitional power. This is indicated by the results that were apparent all through the experiments, which are plainly due to fatigue. The most probable

explanation of such fatigue is the loss of stored energy in the brain cells. In tests of endurance it might be possible to separate the two factors which seem to be parallel in the tests involved in our experiments.

There are, moreover, certain considerations which seem to indicate the presence of another factor, one that is probably present under whatever conditions the practice is carried on. I have called this factor the physiological factor, or cross-influence.

In the tapping experiments[1] it seemed to me highly improbable that a strong will was of any aid to the subject. In fact, the men seemed to tap more rapidly the less attention they paid to the movement. As several expressed the fact, they only needed to "set the machinery going and it went itself."

Observations were made, too, on F. who practiced with the right foot only.[2] F. was a strong healthy man, a trained and skilled gymnast. During 2½ weeks of practice he was not able to make any marked gain. He himself said: "If I try to hurry too much my foot stops almost altogether." His heavy gymnastic work, in which strong effort is necessary, had so developed his ability to send down to large muscles, immense stimuli for action, that for a test involving small muscles he could not become expert.

To discover if these observations would be supported by actual experimental results, I arranged the apparatus so that the subjects might tap with both hands and both feet, though the tapping of but one member was recorded. A sufficient number of records were taken to merit the conclusion that, after a few days of practice, enough to take away the awkwardness due to thinking of so many members, the right hand could tap even more rapidly when tapping together with the other members than when tapping alone. It is probable that when the attention was paid entirely to one member's tapping, the motor impulse was too concentrated for the small muscles: or else the large impulses, overflowing into other muscles, caused contractions that hindered the free movement of the muscles concerned in the movement. This surcharging of the small muscles was relieved when the attention, and so the impulse, was directed to more than one place.

These observations and results lead us to conclude that the tapping test is not one of will power as the term is commonly understood. Strong effort and attention are hindrances to rapid tapping.

Neither is tapping a test in which rapidity is specially dependent on the learning of any knack. The test is a very simple one, involving

[1] Davis, *Researches in cross-education*, Stud. Yale Psych. Lab., 1898 VI 44.

[2] Davis, as before, 12, 13.

only a slight movement of the member tapping, so that no complex coordination of mind and muscle is necessary. The movement is at least semi-automatic and the test is one of simple motor ability.

If, then, the gain in the rapidity of tapping is not due to either the development of volition or of coördination what must the factor be that causes the gain and its transference to the unpracticed members? A little light is obtained from an observation of the test records and practice conditions of an individual subject. The results of this subject's practice were not exhibited in the general table for reasons which will now be understood.

Among the men tested at the beginning of the experiment was one, S.,

TABLE XI.

An individual record. (Subj. S., No. 15.)

	Initial.		First Final.		Gain.	
Subj.	R.	L.	R.	L.	R.	L.
15	33¾ kg.	18¼ kg.	58½ kg.	39¼ kg.	73.3%	115.07%

whose grip was far below the average for men. The left hand recorded only 18¼ kg., right 33¾ kg.; whereas the average for the men of this experiment was 46½ kg. for left hand and 50 kg. for right. That is, his left hand recorded 39.5%, his right 67.1% of the average grip of the twenty-five men subjects. There was moreover no apparent reason for such a low record. S. is a strong rugged man 19 years old and seemingly should have made an excellent record. However, some close questioning brought out a few interesting and important facts.

Two years before the test the metacarpal bone of S.'s left thumb had been broken. About the same time in a second accident the fourth finger of his left hand had been so cut that the long extensor tendon had been separated from its attachment on the distal phalanx, leaving the flexor tendon unopposed. A thorough examination by a physician strengthened the idea I had already formed that the hand had not properly healed after the accidents. So it is very easy to explain the weakened grip for the left hand. Why the grip of the right hand should also be weakened is not so easily explained.

As with the other subjects the practice of S. was continued for three weeks. Having been placed in Group I. his practice was vigorous, right-handed, with the dynamometer. At the final test, records of both hands were taken. Table XI. shows the remarkable gains that were made. The right hand gained 73.3%; the left, 115.0%. At this time the right hand was stronger by 17% than the average right-hand grip for the

subjects experimented with. The left increased from 39.5% to 86.5% of the average. At the second final test the left recorded 90.8% of the average grip.

It would seem rash to attribute this remarkable gain to the development of either volition or coördination. We are at a loss to explain it satisfactorily. Taken in connection with the conclusions reached about tapping it seems probable that there is an additional factor here, that, working in and through the other factors, is directly responsible for the transference of practice effects to the unused side of the body: stated more definitely to those parts of the body that are symmetrical to the parts used or else closely related to them in function or position. To put the proposition baldly this factor is one whose development makes it possible for the other factors, volition and coördination, to become effective in stimulating to a greater degree and governing in a more effective manner, the muscles of the unused side. This development probably accompanies physiological changes in the motor nerve centers in both halves of the brain. If the right hand is used, certain changes are wrought in the right side of the brain to accompany those caused directly on the left side. Whether this change is an actual laying down of potential energy in the motor nerve cells or only a clearing or enlarging of pathways of nervous impulse to the muscles involved, there is at present, of course, no means at hand of knowing definitely. The whole matter is in a measure conjectural. We only know that there is a close nervous connection between the two halves of the brain and that the physiological changes might very readily occur, and in the light of the facts noted it seems most probable that they do really occur.

With the aid of this theory of transference, subject S.'s case may be partially explained. His failure to use his left hand after the accident and the failure of certain parts to properly heal had caused a partial atrophy of the motor center governing that hand. This mal-condition had been shared in by the right hand so that its strength of grip actually weakened, though in a degree not equal to the weakening of the left. Then, when the practice was begun with the right hand, the motor center governing that member quickly responded to practice, gained all it had lost through the accident, and this improved condition had been communicated to and shared in by the left hand's motor center. Or, if the changed condition in the nervous apparatus governing the right hand was simply a clearing up of old pathways that had been clogged as a result of the accident, this changed condition had, nevertheless, been shared in by the nervous apparatus governing the left hand.

The path-clearing theory derives added importance from its considera-

tion in connection with WISSLER's theory of the diffusion of the motor impulse. For if there is a diffusion of impulse to symmetrical muscles the traveling of the impulse to the muscles will wear the paths deeper and deeper and so make a clear way for the impulse when it is voluntarily sent this way at the final test.

The fact that S.'s grip for the right hand had weakened by an accident directly affecting only the left raises the question whether the reverse of cross-education is true; whether a mal-condition of the governing apparatus for the one side is generally transferred to the other side of the brain, and so causes a weakening of the other side of the body. Speaking theoretically it seems reasonable to suppose such to be the case. It would be interesting in this connection to study pathological cases where, for any reason, one member has fallen into disuse, to determine whether the symmetrical member has been weakened. The difficulty would be to find cases that had not used the well arm *more* in consequence of the other's loss of power. Such conditions would of course nullify any results secured.

It must not be understood that any explanation of the causes of cross-education can be applied to all individuals or even absolutely to any individual. Even provided that all the conditions of practice could be made precisely the same, no two individuals would give the same results in development.

Again, any results of practice are modified by the kind of test that is made. Tests of endurance differ very materially from tests of strength. The development of the factors of practice may be required in entirely different proportions in the two kinds of tests. The summary we present here holds only for tests of strength and, even more specifically, only for practice with the dynamometer.

The *central effects* of practice are emphasized as most prominent in this investigation. It is quite probable that the peripheral effects are more necessary of development in tests of endurance than in tests of strength. In the same test we believe it quite possible that the central and peripheral effects may vary in different individuals.

This investigation gives prominence to the following factors of cross-education.

(*a*) *Volition or will power.*—When developed for one act it is efficacious for similar acts done by symmetrical or closely related muscles; in a lesser degree it is possibly developed for all acts.

(*b*) *Neuro-muscular coördination or "knack."*—When developed for one set of muscles it is transferred across to the symmetrical set on the other side of the body.

(*c*) *Potential energy* must be stored in the motor nerve cells in increased amount if a gain in strength is make. To be effective for the unpracticed side there must probably be a storing of energy in the cells governing this side.

(*d*) *Cross influence.* We simply emphasize what has been said under (*b*) and (*c*). The development of coördination and the storing of energy is of no use to the unpracticed muscles unless it takes place, in a measure, in that half of the brain governing those particular muscles. So, too, the clearing up of nervous pathways on the unpracticed side is of probable importance. This cross influence is theoretically possible owing to the close nervous connection between the two halves of the brain. The facts secured from this and other similar investigations point to this direct cross influence of one side of the brain on the other.

(*e*) If there is a peripheral transference of practice effects that is really effective for strength, skill or endurance it can be explained theoretically on the grounds of WISSLER and RICHARDSON'S diffusion of the motor impulse.[1] That there is such an effective transference remains to be proven.[2]

II. INFLUENCE OF TEMPERAMENT ON PRACTICE EFFECTS.

The tables arranged in this section and the conclusions drawn from them grew out of an observation made concerning the development of two subjects, Nos. 22 and 32, the results of whose practice were averaged with those of the rest of the subjects in Section I. A comparison of these results showed such a wide difference among them that the conditions of practice were at once examined to determine a cause for such difference. The subjects were brother and sister, which fact would of itself incline one to classify them similarly in respect to temperament. Appearances would indicate that they are of the same general type. Both are small of stature, quick and active in movement. They were

TABLE XII.

A comparison of individual results. (Subjects 22 and 32.)

Subject.	Method of practice.	Gain, first final.
22	Light, with the dynamom.	9.39 %
32	Vigorous, with the dynamom.	—3.06 %

classed as nervous. Table XII. compares the conditions and results of their practice. The methods of practice differed in that No. 22 exercised

[1] WISSLER AND RICHARDSON, *Diffusion of the motor impulse*, Psych. Rev., 1900 VII 29.

[2] DAVIS, *Researches in cross-education*, Stud. Yale Psych. Lab., 1898 VI 23.

lightly while No. 32 exercised vigorously. The percentage of gain of Subject 22 was rather large, 9.39 % at the first final, compared with the loss of 3.06 % of Subject 32. Since the other conditions of practice were practically the same for both subjects, it seemed rational to attribute the difference in their development as due to the difference in the severity of the practice. Looking further among the subjects it became evident that the great variation of the results of practice was caused chiefly by the various combinations of temperament with severity of practice. Persons of one type of temperament seemed to demand a certain degree of exertion in their exercise in order that they might make good gains in development. It will be our purpose in this section to investigate the influence of temperament on the effects of practice, and the influence of the severity of the practice on development in persons of certain types of temperament; also, to determine if the type of temperament has any influence on the fact or amount of transference of practice effects to the unused side of the body.

An incomplete discussion of the classification of temperaments was given above, p. 69. A few considerations that are of a secondary nature are also of importance in the determination of the type of temperament. The following scheme presents a rather complete view of those considerations that were of either primary or secondary importance in our classifications.

Considerations in the determination of a classification of temperaments.

	Primary.	Secondary.			
Type.	Muscular reaction.	Muscular reaction.	Mental reaction.	Height.	Weight.
Nervous.	Quick	Weak. Medium. Strong.	Quick.	Short.	Light.
Motor.	Average	Weak. Medium. Strong.	Medium.	Medium.	Medium.
Phlegmatic.	Slow	Weak. Medium. Strong.	Slow.	Tall.	Heavy.

It is not contended that this scheme is absolute. It is not complete or comprehensive. In the determination of any individual's temperament other considerations may enter. Moreover a particular individual may have only a few of the secondary characteristics attributed to his type. A nervous person may be tall or heavy. He might even be slow to grasp mental ideas. It is only asserted that all these characteristics were noted and that the subjects when classed as either nervous,

TABLE XIII.

Influence of temperament.

Men.

Subject.	Group.	Temperament.	Gain. 1st final.	Gain. 2d final.
1	I	nervous.	0.48%	2.88%
2	II	phlegmatic.	12.22	13.33
3	III	phlegmatic.	8.41	10.28
5	V	motor.	——	7.75
6	I	phlegmatic.	26.48	——
7	II	nervous.	12.05	15.06
7½	III	phlegmatic.	10.20	4.90
8	IV	mot. phleg.	2.47	——
9	V	phlegmatic.	11.00	7.15
10	I	motor.	3.67	2.29
11	II	phlegmatic.	12.30	10.53
12	III	nervous.	3.45	5.52
13	IV	phlegmatic.	11.60	——
14	V	nervous.	11.12	3.06
16	II	phlegmatic.	—0.47	1.89
17	III	phlegmatic.	—2.08	3.12
18	IV	nervous.	—7.86	—7.86
19	V	motor.	—3.20	——
20	I	phlegmatic.	20.18	25.92
22	III	nervous.	9.39	——
23	V	motor.	6.22	7.88
24	II	motor.	17.39	18.63
25	IV	nervous.	—2.22	0.98
Average,			6.94	7.40
Average of 1st and 2d finals,				7.17

Women.

Subject.	Group.	Temperament.	Gain. 1st final.	Gain. 2d final.
26	I	nerv. mot.	12.50%	7.69%
27	II	phlegmatic.	——	13.06
28	III	mot. nerv.	6.31	10.52
29	IV	phleg. mot.	—0.69	—0.69
30	V	phleg.—moderate.	4.20	6.81
31	I	phlegmatic.	9.00	12.82
32	I	nervous.	—3.06	3.82
33	III	phleg.—moderate.	21.20	6.89
34	IV	nerv. mot.	7.50	11.66
35	V	nerv. mot.	—1.66	9.98
36	I	mot. phleg.	1.42	10.00
37	II	nervous.	20.00	18.88
38	III	motor.	2.63	12.28

39	IV	phlegmatic.	— 5.31	— 2.65
40	V	mot. nerv.	10.15	26.81
41	I	nervous.	5.21	— 1.04
42	II	mot. phleg.	0.78	9.37
43	III	motor.	10.68	25.38
44	IV	nerv. mot.	9.82	12.50
45	V	motor.	15.70	14.21
47	V	motor.	2.78	0.24
48	II	mot. nerv.	1.61	4.83
49	III	nervous.	5.21	6.25
50	IV	phlegmatic.	3.20	— —
Average,			6.05	10.74
Average of 1st and 2d finals,				8.39

motor or phlegmatic would more often than otherwise possess those secondary characteristics attributed to his type. It is quite probable too, that although the classification was based, primarily on the celerity of movement exhibited by the subject yet it might easily have been affected by the influence, either conscious or unconscious, of the secondary considerations.

The results of the experiments of the preceding section were studied in reference to temperament. It only needs to be explained how the individual determination of temperament was made. This was done by the directors of the two gymnasiums. This gave due prominence to the consideration of muscular reaction. It is believed, too, that teachers of gymnastics have a most excellent opportunity to become thoroughly acquainted with a student's characteristics. The determination of the subjects' temperaments was made *before*, not *after*, the beginning of the experiment.

Table XIII. presents the subjects, the group in which each practiced, the type of temperament under which each was classed and the percentage of gain in development each made. The gains shown in the table are those made by the hand practiced. In Group V. an average was made of the gains made by the two hands, since both were practiced.

The table shows that the men gained 6.94% at the first final, 7.40% at the second final. The women gained 6.05% at the first and 10.74% at the second. By averaging the gains of the two final tests it is found that the men gained 7.17%, the women, 8.39%. The average development for men and women was 7.78%.

We notice a very great individual variation among the subjects. The largest gain was made by No. 40, who gained 26.81%. This subject is of a nervous-motor temperament and practiced both hands lightly. Among the men, two subjects, 6 and 20, gained more than 20% ; both

of these are of phlegmatic temperament and practiced vigorously. Among the women, No. 43 gained 25.38%. Her temperament is motor, practice light.

The men gave six minus results out of a total of 40 ; the women gave 7 minus results out of a total of 46. If all those minus results secured from practice with the cylindrical stick are cast out, then all the others, with but two exceptions, one for men and one for women, were the results of two methods of practice ; (*a*) phlegmatic subjects practicing lightly and (*b*) nervous subjects practicing vigorously. The two exceptions were No. 19, motor, and No. 35, nervous motor, both practicing lightly with both hands. So that if we consider, outside of Group IV., only those subjects who were classed as strictly nervous or phlegmatic, then *losses were recorded only for those phlegmatic subjects who practiced lightly and those nervous subjects who practiced vigorously.*

As in Table VII., this new arrangement of results (Table XIII.) shows the effect of fatigue, that decreases in the period of time between the two tests. The fact is more marked among the women than among the men.

TABLE XIV.

To show conditions favorable to gain in strength.

I. Men.

Subject.	Group.	Practice.	Temperament.	Gain. First final.	Gain. Second final.
6	I	vigorous.	phlegmatic.	26.48 %	
7	II	light.	nervous.	12.05	15.06 %
12	III	light.	nervous.	3.45	5.52
20	I	vigorous.	phlegmatic.	20.10	25.92
22	III	light.	nervous.	9.39	
		Average,		14.29	15.50
		Average for both finals,			14.89

II. Women.

Subject.	Group.	Practice.	Temperament.	Gain. First final.	Gain. Second final.
31	I	vigorous.	phlegmatic.	9.00 %	12.82 %
37	II	light.	nervous.	20.00	18.88
49	III	light.	nervous.	5.21	6.25
		Average,		11.40	12.65
		Average for both finals,			12.02
		Average for men and women,			13.45

Table XIV. was arranged to show the results of training when there was a combination of conditions *favorable* to the development of strength. Only the records of nervous and phlegmatic subjects are shown; moreover the nervous subjects must have practiced lightly and the phlegmatic ones vigorously.[1]

The results prove very conclusively that the conditions here present were favorable to the development of strength. There were no minus results obtained: the gains were all fairly large and the averages well above the general averages in Table XIII. The gain per cent. for men was 14.89 %, more than twice as much as the general average. The ratio in the case of the women is 1 : 1.4. The ratio for men and women between the average gain and the gain made under favorable conditions, as shown in Table XIV. is 1 : 1.7.

The results of training will now be noted when there was a combination of conditions *unfavorable* to the development of strength. Here are included only those results secured from nervous persons exercising vigorously and phlegmatic persons exercising lightly. Table XV. shows the individual results and the averages.[1] Among a total of ten records

TABLE XV.

To show conditions unfavorable to the development of strength.

Men.

Subject.	Group.	Practice.	Temperament.	Gain. 1st final.	Gain. 2d final.
1	I	vigorous.	nervous.	0.48%	2.88%
16	II	light.	phlegmatic.	—0.47	1.89
17	III	light.	phlegmatic.	—2.08	3.12
	Average,			—0.69	2.66
	Average for both finals,				0.98

Women.

Subject.	Group.	Practice.	Temperament.	Gain. 1st final.	Gain. 2d final.
32	I	vigorous.	nervous.	—3.06%	3.82%
41	I	vigorous.	nervous.	5.21	—1.04
	Average,			1.07	1.39
	Average for both finals,				1.23
	Average for men and women,				1.10

there were four minus results. The greatest gain is 5.21 % which is less than the general average (Table XIII.). Moreover Table XIV. shows only one gain per cent. that is less than the greatest gain per cent. found in Table XV.

[1] Physically trained men and women were excluded from Tables XIV. and XV. for reasons shown on pages 92 and 93.

The average gain under unfavorable conditions for men and women is 1.10%. The ratio of this gain to the general average gain for men and women (Table XIII.) is 1:7; and to the average gain under favorable conditions (Table XIV.) is 1:12. That is, there is a probability that there will be twelve times as much development in a nervous person exercised lightly, than if he exercised vigorously. The reverse may be said of the plegmatic subject.

It was noted that if subjects of the motor or phlegmatic type had had considerable previous physical training they required *light exercise* to secure large gains in development of strength. It seemed as if physical training had modified their temperament in so far at least as the time of their reaction to the specific stimulus of exercise was concerned. Table XVI. presents the fact very clearly. In arranging the tables only motor and phlegmatic types were included and only those subjects that had had a somewhat extensive physical training previous to the beginning of the experiment. Another necessary requirement was light practice during the experiment.

TABLE XVI.

To show the effect of previous physical training on temperament.

I. Men.

Subject.	Group.	Practice.	Temperament.	Previous training.	Gain. 1st final.	Gain. 2d final.
3	III	light.	phlegmatic.	track.	8.41%	10.28%
5	V	"	phleg.-mot.	"		7.75
7½	III	"	phlegmatic.	"	10.20	4.90
9	V	"	"	general.	11.00	7.15
11	II	"	"	"	12.30	10.53
23	V	"	motor.	"	6.06	7.03
24	II	"	"	track.	17.39	18.63
		Average,			10.89	9.75
		Average for both finals,				10.32

II. Women.

Subject.	Group.	Practice.	Temperament.	Previous training.	Gain. 1st final.	Gain. 2d final
27	II	light.	phlegmatic.	4 yrs.		39.06%
43	III	"	motor.	4 yrs.	10.68%	25.38
45	V	"	"	3 yrs.	15.75	14.21
47	V	"	"	7 yrs.	2.88	0.37
		Average,			9.77	19.75
		Average for both finals,				14.76
		Average for men and women,				12.54

The final averages are considerably above the general averages (Table XIII.); 12.54% as compared with 7.76%. The gains made by the subjects of Table XVI. become significant when compared with the gain of subjects working under unfavorable conditions (Table XV.), 12.54% to 1.10%. The fact is illustrated again in Table XVII.

TABLE XVII.

Effect of previous training on temperament.

	Temperament.	Practice.	Previous training.	Gain. 1st final.	Gain. 2d final.	Gain. Average.
a	phlegmatic.	light.	considerable.	10.48%	14.36%	12.42%
b	phlegmatic.	light.	none.	— 1.27	2.50	0.61

In arranging this table only phlegmatic subjects were included and the practice was, in all cases, light. But the subjects in line *a.* had had considerable previous physical training; those in line *b.* had had none. The ratio between the average results, 12.42 and 0.61 is 20:1. That is, those phlegmatic subjects who were physically trained men and women made 20 times as great a development of strength as those who were not so trained, both classes practicing lightly. It is evident, therefore, why those phlegmatic subjects who were trained men and women physically were excluded from Tables XIV. and XV.

What has just been said about the effect of previous training is probably only a specific effect. This effect is analogous to that produced on the neuro-muscular system as the result of training in quickness of simple reaction. A decrease in simple reaction time results only when all those physiological elements which are concerned in converting a stimulus into muscular movement learn to respond more quickly to that stimulus. So exercise must be considered a stimulus to development since it accelerates the activity of those organs whose activity is necessary to the building up of muscular and nervous tissue. If a phlegmatic subject has had a motor training his physical system has gained the power or habit of reacting more quickly to the stimulation of light exercise and rapid growth follows more rapidly than could otherwise have happened.

Table XVIII. was arranged for the purpose of a summary and comparison of the facts brought out in this section. A embraces all subjects of whatever temperament or whatever their method of practice. The gains per cent. are general averages. B includes all the nervous subjects who practiced lightly, or the phlegmatic ones (except those affected by previous training) who practiced vigorously. Such conditions are favorable to development and a large percentage of gain was recorded for this class. In C the

subjects practiced under unfavorable conditions, just the reverse of B, and the percentage of gain was small. In D are those motor and phlegmatic subjects who had had considerable previous physical training, and who, by virtue of this, made large gains in strength by light practice.

TABLE XVIII.

Comparison of the effect of different conditions on the results of practice.

Men and Women.

Subjects.	Conditions.	Temperament.	Practice.	Gain. Men.	Gain. Women.	Gain. Average.
A.	all	all	all	7.17%	8.30%	7.78%
B.	Favorable to gain.	Nervous Phlegmatic	Light Vigorous	14.89	12.02	13.45
C.	Unfavorable to gain.	Nervous Phlegmatic	Vigorous Light	0.98	1.23	1.10
D.	Effect of previous training.	Phlegmatic Motor	Light Light	10.32	14.70	12.54

A fact worthy of relation was noted in the case of No. 47 ; inasmuch as the result of her practice seemed to be an exception to the general principles discovered. No. 47 was classed as of motor temperament and practiced lightly with both hands. Moreover, she has had seven years of gymnastic training. Judging from the principles enumerated conditions seemed favorable to the development of strength. But the gain was slight (see Table XIX.).

To determine whether development could be secured by a different method of practice, two months after the completion of her final test exercise with the dynamometer was again begun. At this second period of practice, work was made much heavier, since we had concluded that a mistake had been made in classifying the subject at the first test. Work was now made suitable for the development of a phlegmatic rather than a motor person. The results of this practice were strikingly different

TABLE XIX.

Effect of different methods of practice on one individual's results.

Subject.	Temperament.	Practice.	Gain. 1st final.	Gain. 2d final.	Gain. Average.
47	?	Light.	2.78%	0.24%	1.51%
47	?	Vigorous.	12.04	6.85	9.42

from the first ones. The results of both methods of practice are shown in Table XIX. The vigorous practice effected much the greater development. This substantiated our second classification of the subject as phlegmatic.

The subject's phlegmatic tendency is shown, too, when a comparison is made of the gains at the first and second finals. She did much better at the first final than at the second whether the practice was light or vigorous. That is, there was no visible effect of fatigue present in either case. This indicates a large reserve of potential energy which is a characteristic of the phlegmatic type of temperament. There is undoubtedly for every individual a method of practice that is best suited to his peculiar characteristics. This method differs in intensity or amount from that which is best suited to any other individual. That amount of exercise must be given that will be just sufficient to stimulate physiological processes of growth but always stops short of the fatigue point.

There can be no precise division of individuals into classes. While we have classified all persons under one of the three types of temperament yet it must not be supposed that these three types are separated from each other by distinct lines of demarkation; or that all persons belong absolutely to one type. The types merge into one another; a certain individual may be on the border line between two types. It was in accordance with this idea of temperament that our classification was discussed as it was. The motor temperament is only an average between the other two, simply a collection of the medium colors in the shading from the deepest phlegmatic to the faintest nervous type.

No attempt will be made at this time to explain completely those facts that we have demonstrated to be true in the first pages of this section. The field is a new one and too little work of an experimental nature has been done to merit any well-founded conclusions. We are warranted, however, in stating hypothetically those conclusions that seem to be supported by facts brought out by our experiments.

We have considered exercise as a stimulus to growth. The repeatedly contracted muscle increases in size. So must any other part of the neuro-muscular system be exercised to develop into its perfection of function. By the use of any part of the body more blood is drawn to that part; worn-out material accumulates and is more rapidly carried away; and new material is laid down faster than would otherwise happen. The metabolism is enforced. Now, if exercise is too light, the vital functions are not sufficiently stimulated to work the desired change. If the work is too violent or too long continued the tearing down more than counteracts the building up process and development of the parts used is impossible. If the work is just right in intensity and amount the anabolism provoked is greater than the katabolism and there is development of the parts used.

A review of these facts makes plain what an almost endless variation of conditions would be necessary to make the adjustments of exercise suitable to all individuals. Exercise must be prescribed *per order* just as a dress must be fitted to the individual.

All persons may be classed into the three types of temperament in reference to the time of their muscular reaction to outside stimuli. Nervous persons are nervous in the sense of our consideration because they react quickly to the stimuli of their environment. To get equally quick reactions from the three types, the stimulus must be varied from very strong to very weak. So, considering exercise as a stimulus to the activity of the vital functions, it seems hardly necessary to state the further and essential analogy that while a little exercise is sufficient to bring about these changes in some persons, in others it requires a great deal. The nervous man's heart action will be accelerated to a very appreciable degree when he rises from a lying to a standing posture. An equal effect may be wrought on a phlegmatic person only after considerable work. The immediate effects of exercise may then be classed with all responses of the physical body to direct stimulation.

Why some persons should voluntarily or involuntarily react more quickly than others to a given stimulus, probably depends on what we may term the balance of those factors making up the neuro-muscular system. The differing effects of fatigue visible in different persons either after a daily practice, or after a long practice period point to the importance of two or three factors, whose presence in varying amounts or intensity determines the type of reaction or temperament.

Other factors being equal the smaller muscle will react more quickly than the larger one, because it can be stimulated more easily by a motor impulse of definite intensity. On the other hand, the large muscle contains more reserve material, and for this reason and because this reserve is more slowly used up, the larger muscle is more enduring.

We can apply the same principles to the motor cells of the brain. The larger they are and the more potential energy they contain the more slowly is this potential energy converted into the kinetic energy of the nervous impulse, and, by virtue of this very reason, the more enduring is their strength.

The greater the volitional strength, the quicker and the more thoroughly can the motor impulse be generated from energy present in the brain cells.

The nervous subjects seem then to be characterized by lack of much reserve energy either in muscle or nerve cell, but they are able to more thoroughly utilize the energy at hand. Phlegmatic subjects are slow be-

cause they have much reserve energy but cannot utilize it quickly. In the typical motor subject the several factors are evenly and nicely balanced. More precise and carefully adjusted action should be expected of this type of persons.

Too little experimental work has been done to merit any pronounced or well founded conclusions about the relations of temperament and training. Observation of the results of our own experiments in this direction seems to point to the truth of the following:

(*a*) Nervous persons, in training for the development of strength, require light practice.

(*b*) Phlegmatic persons require vigorous practice.

(*c*) Motor persons are an average between these two.

(*d*) Previous physical training works such a change in the phlegmatic person that thereafter he requires less vigorous work to secure large gains in development of strength.

(*e*) It is probably true that the phlegmatic type of temperament is characterized by the presence of much reserve energy of muscle and nerve cell. The nervous type has less reserve energy but a greater ability to use the energy at hand.

It is not difficult to apply the principles stated above to practical physical training. They make necessary on the part of the trainer a personal knowledge, secured either by means of observation or experiment of the temperament of each man under his charge. The amount of work necessary in each case can than be apportioned with much greater exactness.

It seems quite as certain that there may also be a direct application of these principles in the realm of pedagogy. Our experiments show that, in the development of strength, mental factors are more necessary than muscular factors. In fact our conclusions relate as much to one set as to the other. If we can apply the principles to the development of will power and coördination why not to memory, association, imagination and reasoning as well. All have a physiological basis and in so far all are governed, in a given individual, by the same principles of growth.

There is at least a wide field here for inquiry and practical investigation. Our present system of secondary and collegiate instruction requires an equal amount of work from all pupils. All must measure up to a common standard. There can be no doubt that such a system results in much harm to many individuals.

The results of this section emphasize the importance of recognizing the individual in the training of either physical or mental ability.

III. Experiments with the ergograph.

The ergograph used was similar in construction to Mosso's.[1] The weight raised was 2½ kilos. It was lifted by the middle finger, the first and third fingers being encased in stationary brass tubes lined with felt. This arrangement enforced a simple movement of the finger used. The record was made on a recording drum in the usual way.

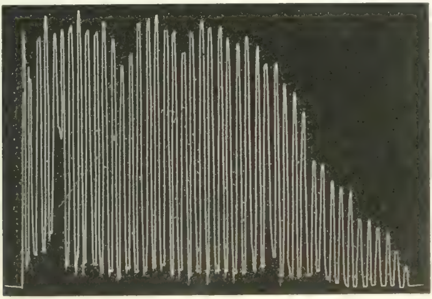

Fig. 1.

The curves shown in Figs. 1 to 7 are records of the ability of Subject X. He was 26 years of age, well trained physically and of the motor temperament. The initial record for the left hand is shown in Fig. 1, the total distance through which the weight was raised being 1341 ^{mm}; the inital

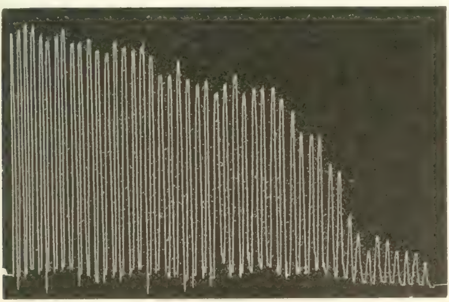

Fig. 2.

record for the right hand is shown in Fig. 2, the total distance being 1316 ^{mm}.

[1] Mosso, *Ueber die Gesetze der Ermüdung*, Arch. f. Anat. u. Physiol., Physiol. Abth., 1890, 89.

These records were made on March 1, 1899. Every day thereafter, for three weeks, with only two exceptions, the left hand was practiced. This practice consisted in completely fatiguing the hand once each day by pulling at the ergograph. A record of each day's work was preserved

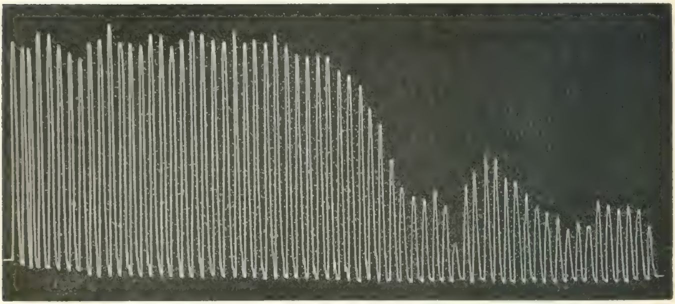

FIG. 3.

on smoked paper. On March 23d final records were secured for both hands. These records are shown in Figs. 3 and 4. The record for the left hand after three weeks of vigorous practice shows (Fig. 3) a total distance of 1488 ^{mm} the gain over the initial record for the left hand being

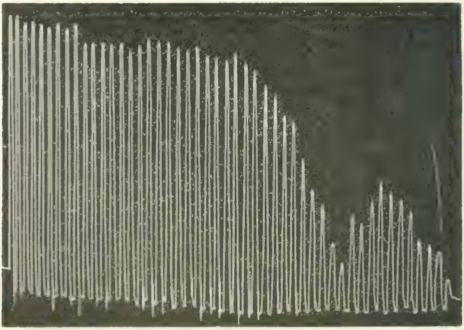

FIG. 4.

11%. The record for the right hand after three weeks of vigorous practice of the left shows (Fig. 4) a total distance of 1492 ^{mm}, the gain over the initial record for the right hand being 13%.

The results secured from this practice were not satisfactory. It seemed that larger gains in development of strength should have been secured. Therefore on April 1, after a rest of one week, practice was again begun but was made light instead of fatiguing. This practice was continued

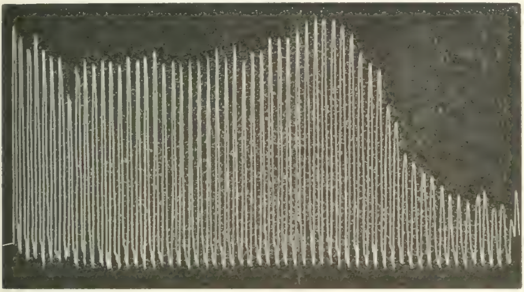

FIG. 5.

for seven weeks, the *left* middle finger alone being exercised as before. Then two final records were taken. These are shown in Figs. 5 and 6.

The record of left hand (Fig. 5) five weeks after that in Fig. 3 shows a total distance of 1757^{mm}, the gain over that shown in Fig. 3 being 18%.

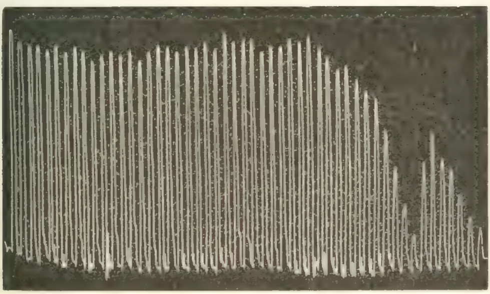

FIG. 6.

The record of right hand (Fig. 6) five weeks after that in Fig. 4 shows a total distance of 1541^{mm}, the gain over that in Fig. 4 being 3%.

The percentages of gain both from the heavy and from the light practice are shown in Table XX. The short time of vigorcus practice

brought about results in development similar to the average results secured from the practice with the dynamometer. The effect of fatigue is seen

TABLE XX.

The development secured from practice with the ergograph.

	Distance weight was lifted in mm.	Distance after three weeks' vigorous practice.	Gain.	Distance after seven weeks' further practice. Practice light.	Gain over first final.
Left hand.	1341	1488	11 %	1757	18 %
Right hand.	1316	1492	13 %	1541	3 %

in the greater gain of the right hand which was not practiced.

A marked difference is seen between the results secured from the two periods of practice. The right hand made very little gain from the second period. It is not certain however that this difference resulted from the difference in the methods of practice. It is extremely probable, rather, that the subject had reached a point in his development when the effects of practice were not transferred in any marked degree to the unused side of the body. Let us examine the method of practice and results of this subject with this idea in view.

In his daily practice his left hand was required to do nearly as much work as in the first practice but the work was done between intervals of rest. For example on April 11, the left finger made 35 full contractions but made them in three periods with two short rests between the periods.

The final records of Subject X. shows that the left hand had gained 18 % over its record made at the end of the first practice; the right, only 3 %. This fact, if it is considered in connection with all the other facts noted in the previous sections, can be explained in only one way. The left hand, after over 8 weeks of light and heavy work, was at the end of that time getting all of the benefits of exercise. The right was getting none. This fact proves that the unused side does not always get a share in the benefits of exercise. One side, by long continued practice, may become over-developed while the strength of the symmetrical muscles on the other side may even be diminished. It is possible that the failure of some of the subjects to make a gain in dynamometric strength may be explained in the same way. This principle would be an interesting one to investigate thoroughly and completely. At present we can assert only hypothetically that long continued exercise of one member causes its development out of all proportion to that of the symmetrical member on the other side of the body.

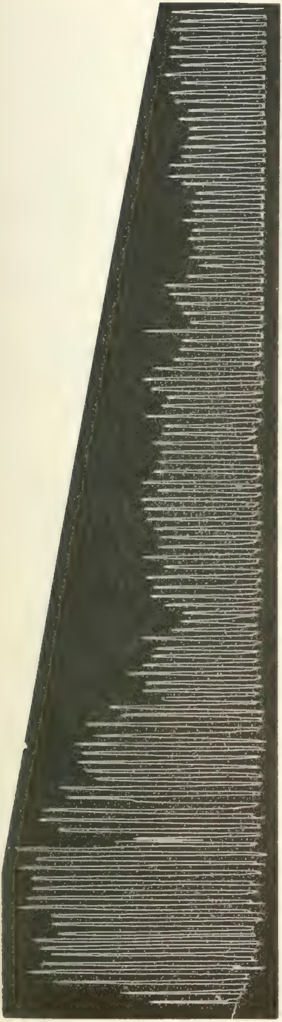

FIG. 7.

FIG. 8.

The curves shown in Figs. 7 and 8 illustrate a fact noted in the curves of all of the subjects: that the right and left hand curves of any individual are very similar in form. Subject J. was left-handed. Both curves exhibited the same general form of outline. Both are rather irregular, growing more regular toward the last. In both, the first contractions are short, then there is an increase for several contractions and finally a gradual decrease.

In the same connection should be examined the curves shown in Figs. 2 and 9. Both are records of Subject X.; Fig. 2, the right initial, Fig. 9, the third practice of the left hand. The outlines of the two curves are almost precisely the same. These observations indicate the paramount importance of the central influences affecting the strength of muscular contractions.

Subject X. was able to completely exhaust his store of energy. Subject J. did not have this ability. The fact is shown in the abrupt endings of his curves. The curves in Fig. 7 and 8 end more gradually. Tem-

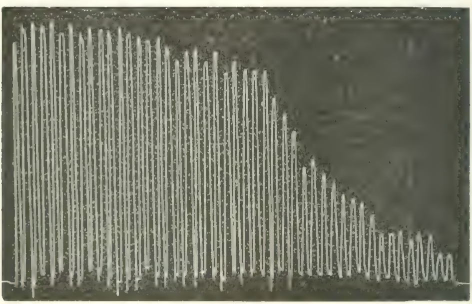

FIG. 9.

perament and training both have an influence here. X. is motor in temperament and has had an excellent training in athletic sports. J. is phlegmatic and has had very little physical training. We have already said that a characteristic of the phlegmatic temperament was the lack of ability to command the use of the energy stored in muscle and nerve cells. It is equally as certain that a training in athletics increases this ability. J.'s curve for the left hand (Fig. 7) was cut short for want of space. With the contractions as short as the last ones the work might have been kept up almost indefinitely. Still J. said that he was exerting his power of contraction to the utmost. He did not have the ability to tire himself out. An athlete of the motor temperament has this ability *par excellence*.

There is a noticeable increase in the regularity of the fatigue curves of Subject X. from the beginning of the practice until the last record. Compare Figs. 1 and 5, both of the left hand. Curve 5, however, was taken after a long practice. The increase in regularity is marked. We may thus conclude that practice perfects muscular control.

Our observations from the study of ergograph curves may be summed up as follows:

(*a*) Long continued practice of one side of the body may develop that side in much greater degree than the unused side is developed. If continued long enough such practice may even detract from the strength of the side not used.

(*b*) The similarity of right and left hand curves proves and illustrates the paramount importance of the influence of the central nervous system on muscular contraction.

(*c*) The long drawn out fatigue curve is caused by the inability of the subject to command the instant use of his nervous energy. Herein lies a chief difference between the phlegmatic and motor or nervous temperaments. It exhibits a difference too between physically trained and untrained men.

(*d*) Practice improves regularity of muscular contractions. This improvement is due, presumably, to better coördination of mind and muscle.

IV. General Considerations.

The meaning and scope of cross-education were stated in my first series of researches.[1] The conclusions drawn there will need modification in the light of recent investigation, although it may be said in general that their essential features are more firmly established than before. The main conclusions were:

"(*a*) The effects of exercise may be transferred to a greater or lesser degree from the parts practiced to other parts of the body. This transference is greatest to symmetrical and closely related parts.

"(*b*) There is a close connection between different parts of the body through nervous means. This connection is closer between parts related in function or position.

"(*c*) Will power and attention are educated by physical training. When developed by any special act they are developed for all acts."

It seems necessary in the consideration at hand to determine precisely where fatigue first appears, whether in the central or peripheral organs. Most observers contend that fatigue, when it is considered as inability to do further work, is due essentially to losses in the central

[1] Davis, *Researches in cross-education*, Stud. Yale Psych. Lab., 1889 VI 16.

nervous system. HODGE[1] has shown by experiment that the normal fatigue of the day is associated with loss of substances in the central ganglion cells. WALLER[2] asserts that the central part of the neuro-muscular mechanism tires first, and hence a protection is afforded to the muscles and end organs. The investigations of LOMBARD,[3] MOSSO,[4] and FECHNER[5] all point in the same direction. The facts of cross-education are explained much more easily on such an hypothesis.

In 1894, KOCH[6] reaffirmed the statements of MOSSO and LOMBARD, that, in general, central fatigue sets in earlier than muscular fatigue. He also states that, after fatigue, the nervous matter regains its normal function more quickly than the muscles. He studied the effect on the ergographic curve of drinking water and of the injection of cocaine and caffeine. He found that changes in the ergographic curve were caused much more readily by central than by peripheral factors. Nevertheless several peculiarities in the caffeine curve, its *gradual* increase and in general the entire curve points perhaps to a direct effect on the muscle itself.

JOTEYKO,[7] by an investigation in which the ergograph and the dynamometer are used alternately, has reached conclusions that bear *directly* on "transference"; and also *indirectly* since they are an addition to our knowledge of the relations of central and peripheral fatigue. Her method of experiment was: (1) to secure records on the dynamometer for both right and left hands; (2) immediately thereafter,

[1] HODGE, *Some effects of stimulating ganglion cells*, Amer. Jour. Psych., 1888 I 479.

HODGE, *Some effects of electrically stimulating ganglion* Amer. Jour. Psych., 1890 II 376.

HODGE, *The process of recovery from the fatigue occasioned by the electrical stimulation of cells of the spinal ganglia*, Amer. Jour. Psych., 1890 III 530.

[2] WALLER, *The sense of effort*, Brain, 1891 XIV 218.

[3] LOMBARD, *The effect of fatigue on voluntary muscular contractions*, Amer. Jour. Psych., 1890 III 24.

LOMBARD, *Some of the influences which affect the power of voluntary muscular contractions*, Jour. Physiol., 1892 XIII 1.

LOMBARD, *Alterations in the strength which occur during fatiguing voluntary muscular work*, Jour. Physiol, 1893 XIV 97

[4] MOSSO, *Ueber die Gesetze der Ermüdung*, Arch. f. Anat. u. Physiol., Physiol. Abth., 1890 89.

[5] FECHNER, *Ueber den Gang der Muskelübung*, Ber. d. k. säch. Ges. d. Wis., math.-phys. Klasse, 1857 IX 113.

[6] KOCH, *Ergographische Studien*, Marburg 1894.

[7] JOTEYKO, *L'Effort nerveux et la fatigue*, Arch. ital. de Biol., 1899 XVI.

JOTEYKO, *Rech. expér. sur la fatigue des centres nerveux par l'excitation électrique*, Soc. de Biol. de Paris, 1899 834.

JOTEYKO, *Rech. expér. sur la fatigue des organes terminaux*, Soc. de Biol. Paris. 1899 386.

to fatigue the right hand by exercise with the ergograph and then (3) finally to again secure records of dynamometric pressure for the right and left hands. She found that the work done by the right hand with the ergograph decreased the dynamometric pressure on the left hand. This decrease was about 20%. She concludes that the decrease in power in the unused hand was caused by the loss of energy in the cerebral centers. Under ordinary conditions however it was found that when the work was not pushed to the limit of endurance, peripheral fatigue appeared first and apparently, therefore, prevented central fatigue. It was only when by an effort of the will the subjects prolonged the physical work in spite of the feeling of weariness in the muscles, that the motor excitation of the muscles increased the fatigue of the nerve cells rather than that of the muscles themselves.

In connection with JOTEYKO's article it is necessary to distinguish between the fact of fatigue and the feeling of fatigue. They have been considered as something entirely distinct and separate. *Sensations* of fatigue are very deceptive, and should be no criterion for the estimation of the *fact* of either mental or bodily fatigue. Fatigue[1] is defined as the decrease in the capacity for work: fatigue in this sense may or may not have definite relations to the peculiar sensation known as the "feeling of fatigue." This is probably true whether said of mental or of physical work. "The[2] current ideas about the *fact* and the *feeling* of mental fatigue, and the relation between them, are naïve abstractions based on a simple minded analogy, a failure to carefully analyze certain mental states, and a confusion in the case of experimental investigations between lack of *desire* and lack of *ability* to work." There is always danger, too, of mistaking the location of fatigue. While it might seem that the conditions causing the feeling of fatigue from muscular work arise in the muscles themselves, yet it is probable that those very conditions are the result of changes taking place in the brain.

WOODWORTH[3] carried out some experiments with various forms of the graphic method, movements being made at any desired speed or interval. Some of his conclusions bear directly on "transference." "Practice of the left hand helped the right also. Before the series with the left hand began, and again after it was completed, a single experiment was made with the right hand. The right hand showed a decided improvement at the rate for which the left had improved but no appreciable improvement at the rates for which the left had not improved.

[1] SCRIPTURE, The New Pschology, 247, London 1897.

[2] THORNDIKE, *Mental Fatigue*, Psych. Rev., 1900 VII 482.

[3] WOODWORTH, *The accuracy of voluntary movement*, Psych. Rev. (Monograph Supplements), 1899 III 105–106.

These results show (1) that the transferences of the effects of practice from one side of the body to the other—a transference which has been established in other investigations as taking place from the right side to the left—also takes place from the left side to the right[1]; and (2) that it is not the mere practice that has this transferred effect but only successful practice." If at any particular speed the left did not gain then the right did not. "When the left hand does gain the right shows the benefits in almost equal measure."

The fact noticed by WOODWORTH that the effects of practice were transferred only when such practice was successful, may probably be stated as a general truth. Individual cases however may show an increase in the ability of the unpracticed side even though the practiced side makes no gain whatever." This fact was especially marked in the case of Subject E. in the tapping test.[2] There was a loss in the right foot (practiced) of 4% and a gain of 38% in the left foot (not practiced).

The experiments of WISSLER and RICHARDSON[3] show to how great an extent motor impulses to action may spread into muscles that are related to the one directly concerned with a certain movement. The subject's hand was put into a light clamping apparatus and tambours placed on the abductor indices, the forearm and the biceps, so that any contraction made by any one of the muscles could be recorded on the kymograph. The object was to determine whether as a result of the primary contractions of the abductor indices, there was any secondary contractions of the muscles of the arm. It was found that there was such a secondary contraction, showing that there was an overflow or diffusion of the motor impulse into adjacent muscles.

It was found also that by the training of the left biceps in strength, definite gains in strength were made by the left abductor, right abductor, left forearm, etc. "It seems certain that the exercise of any muscle reacts upon all related muscles, which is to say that diffusion takes place in both inward and outward directions."[4]

The following hypotheses are then put forward:

(*a*) That the exercise of an accessory muscle has a greater reactionary effect upon the adjacent fundamental muscles than upon the more remote.

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[1] This fact was proven, independently of WOODWORTH'S work, by an investigation with the ergograph which I carried on during March and April, 1899. The results were not published then but appear in this paper now for the first time. (See Section III, p. 97 and also p. 100.)

[2] DAVIS, *Researches in cross-eduction*, Stud. Yale Psych. Lab., 1889 VI 11.

[3] WISSLER AND RICHARDSON, *Diffusion of the motor impulse*, Psych. Rev., 1900 VII 29.

[4] Same, p. 35

(*b*) That an accessory muscle of one arm gains as much from the training of the corresponding muscle of the opposite side as from the training of the fundamental muscle of the same side. In terms of motor discharge this indicates that these centers occupy the same diffusion level.

(*c*) That the reactionary or secondary gain of the fundamental muscles from the exercise of the accessory arm muscles is less than when the conditions are reversed—*i. e.*, the fundamental muscles practiced and the accessory muscles reacted upon. This is in harmony with the accepted order of motor development.

The theory of the spreading of impulses so clearly proven by WISSLER and RICHARDSON had been stated before by EXNER[1] and URBANTSCHITSCH.[2] DAMSCH[3] explained this diffusion as due to the close connection of all motor centers. Fibers from all motor centers come together and are intimately connected in a central ganglion of the brain. The diffusion of an impulse is hindered by an inhibiting apparatus which prevents it from going the wrong way. "This apparatus is much improved by practice. In the young and in certain nervously disordered persons it is deficient."

"It was noticed in the tapping experiment that there was a tendency for the subject's left foot to make movements to accompany those made by the right. In learning an act that involves fine coördination it is very obvious that the pupil executes many movements that are entirely unnecessary."[4] Nearly all the subjects in the experiment with the dynamometer[5] would unconsciously clench the empty hand as well as the one in which the dynamometer was placed.

The consideration of the above facts would seem at first glance to explain "transference" as due to the unconscious indirect exercise of those motor centers, nerves and muscles that are not necessary to the movement that is being performed. It is true that the unused symmetrical or closely related muscles may grow in size as a result of another muscle's exercise. This was shown quite definitely in my first research. But there is a question whether this new substance that has been added to the muscle is a factor in the increased strength, rapidity or endurance

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[1] EXNER, *Zur Kentniss von der Wechselwirkung der Erregungen im Centralnervensystem*, Arch. f. d. ges. Physiol (Pflüger), 1882 XXVIII 487.

[2] URBANTSCHITSCH, *Ueber den Einfluss von Trigeminus-Reizen auf die Sinnesempfindungen, insbesondere auf den Gesichtssinn*, Archiv. f. d. ges. Physiol. (Pflüger), 1883 XXX 129.

[3] DAMSCH, *Ueber Mitbewegungen in symmet. Muskeln an nicht-gelähmten Gliedern*, Zt. f. klin. Med., 1891 XIX 170.

[4] DAVIS, as before, p. 49.

[5] DAVIS, as before, p. 24.

of the unused muscle. The facts observed there[1] seem to point to the conclusion that the substance added to the unused muscles is not a factor in the increased endurance, and, in so far as so few observations can, seem to eliminate the muscular factors altogether from the question of the causes of cross-education.

SCRIPTURE[2] in a recent article has stated the three facts of cross-education as follows:

1. "The principle of cross-education has been proved beyond question."

2. "The gain by practice consists in a development of higher nerve centers connected with the two sides of the body."

3. "The effects of practice are extended to different parts of the body."

Finally he believes that "sooner or later we shall be able to establish the fact that development of those forms of the will involved in simple muscular activities does also develop the more complicated forms that express themselves in acts of a mental nature."

[1] DAVIS, as before, p. 23.

[2] SCRIPTURE, *Cross-education*, Popular Science Monthly, 1900 LVI 589.

COMPUTATION OF A SET OF SIMPLE DIRECT MEASUREMENTS

BY

E. W. SCRIPTURE.

I. THEORY OF THE AVERAGE AND OF REPRESENTATIVE ERRORS.[1]

Given the single measurements x_1, x_2, $\cdots$, x_n where n is a finite number, a good representative value is found by taking the average

$$x = \frac{x_1 + x_2 + \cdots + x_n}{n}.$$

This average is not the true value but merely a representative of the group of particular values. Thus in measuring the simple reaction time of a given subject at a given time under given external and internal conditions the results will not coincide if the unit of measurement is small enough; there may be errors in the apparatus, and there are undoubtedly fluctuations in the internal condition (for example, of attention) that cannot be taken into account in the stipulations. Let the apparatus and the external conditions be made constant and correct to less than two-thirds of a thousandth of a second; the results of the measurements are still irregular and their irregularity can be ascribed entirely to the fluctuation in the internal conditions, that is, to mental sources.

What is the "true" reaction time among all the irregular measurements obtained? The problem is like that of determining the "true" height of a class of students; each individual has a particular height which is true for him but a "representative" height is wanted for the class.

The average is generally assumed as the best representative value. Yet each additional measurement is likely to alter the average; the average for five measurements will probably differ from the average for ten measurements, etc. The simplest assumption is that the ideal average, or true value, is that for an infinite number of measurements. Thus, if

[1] References, for general works: WEINSTEIN, Handbuch d. physikalischen Maassbestimmungen, Berlin 1886 (to which the present account owes much); CZUBER, Theorie d. Beobachtungsfehler, Leipzig 1891; PIZETTI, Fondamenti matematici per la critica dei resultati sperimentali, Genova 1892; BERTRAND, Calcul des Probabilités, Paris 1889; AIRY, Theory of Errors of Observation, London 1875; MERRIMAN, Method of Least Squares, 6 ed., New York 1894.

we could measure the given simple reaction time for the given subject under the given conditions for an infinite number of times, we would have a value that we could well call the "true" value for the given conditions.

True errors.

Let this true value for the set of measurements $x_1, x_2, \cdots, x_n$ be X. The individual measurements would vary from the true value X by the amounts $V_1 = x_1 - X$, $V_2 = x_2 - X$, $\cdots$, $V_n = x_n - X$. These are the "true deviations," or "true variations," or "true errors," of the individual measurements for the true value. The term "error" has in this case no detractory meaning; it is simply the usual expression for a deviation in such a case and, being almost universally adopted, it is probably best to adopt it in psychology also.

Representative errors.

It is nearly always necessary to know the amount of fluctuation of the individual measurements around their representative value. When the true value is used as the representative value, the fluctuations are V_1, V_2, $\cdots$, V_n. A good representative of $V_1, V_2, \cdots, V_n$ would be the average of them all regardless of sign, that is, the average of the amounts by which the single measurements disagree with the true value—not considering kind of disagreement whether positive or negative. Thus

$$D = \frac{|V_1| + |V_2| + \cdots + |V_n|}{n}$$

would be a true average deviation, or true average variation, or true average error, the vertical lines indicating that the V's are used without the signs + or —. Such a value can be called a representative variation or representative error.

The true value of X, however, cannot be known and the average x is usually assumed as the best practicable representative value. The deviations from the average, or the practical errors, are then $v_1 = x_1 - x$, $v_2 = x_2 - x$, $\cdots$, $v_n = x_n - x$. These practical errors are not the same as the true errors. The average of these practical errors without regard to sign is

$$d = \frac{|v_1| + |v_2| + \cdots + |v_n|}{n}.$$

This is what has been improperly called the mean variation, the mean error, the average variation, the average error, or the average deviation. It is, however, merely an approximation to the true average error. If a

closer approximation can be found, this rougher one should be rejected in favor of the better. To find this better quantity we must first consider another representative variation.

Mean error.

Another representative deviation might be found by taking the square root of the average of the squares of the deviations, or

$$M = \sqrt{\frac{V_1^2 + V_2^2 + \cdots + V_n^2}{n}}.$$

This is known as the "mean error," or "mean-square error," or "standard deviation." The term "mean variation" means this quantity and not the previous one, D, which is termed the "average variation."

Since the true value X cannot be known, the true errors $V_1, V_2, \cdots, V_n$ cannot be calculated. The practical errors $v_1, v_2, \cdots, v_n$ can be used to calculate a value

$$m = \sqrt{\frac{v_1^2 + v_2^2 + \cdots + v_n^2}{n}},$$

which is only an approximation to the mean error, or mean-square error. This approximation can be improved in the following way:

The difference between the average and the true value may be called the "resultant error"; it will be

$$K = x - X.$$

It is evident that

$$V_1 = v_1 + K, \quad V_2 = v_2 + K, \quad \cdots, \quad V_n = v_n + K.$$

Substitution of these values in the equation

$$M^2 = \frac{V_1^2 + V_2^2 + \cdots + V_n^2}{n},$$

gives

$$M^2 = \frac{v_1^2 + v_2^2 + \cdots + v_n^2 + 2K(v_1 + v_2 + \cdots + v_n) + nK^2}{n}.$$

But the definition of the average gives

$$v_1 + v_2 + \cdots + v_n = 0.$$

Therefore

$$M^2 = \frac{v_1^2 + v_2^2 + \cdots + v_n^2}{n} + K^2 = m^2 + K^2.$$

This would give the degree of approximation of m to M if K could be known.

We might have considered the difference between the true and the practical errors as separate quantities; thus

$$\delta_1 = V_1 - v_1, \quad \delta_2 = V_2 - v_2, \cdots, \delta_n = V_n - v_n.$$

These values of δ would indicate the precision of the set $v_1, v_2, \cdots, v_n$, as compared with the set $V_1, V_2, \cdots, V_n$ and their mean error would be a good characteristic error for the purpose. This mean error would be

$$\mu = \sqrt{\frac{\delta_1^2 + \delta_2^2 + \cdots + \delta_n^2}{n}}.$$

But $K = \delta_1 = \delta_2 = \cdots = \delta_n$ and thus $\mu = K$. We now have

$$M^2 = m^2 + \mu^2,$$

where μ indicates the closeness of approximation of m to M.

With an infinite number of measurements $x = X$; whereby $K = 0$, and $\mu = 0$. With only one measurement $v_1 = 0$ and $M = \mu$. Thus μ varies between the values 0 and M. Now it is a well established fact that the measure of uncertainty of a set of measurements varies inversely as the square root of the number of measurements. The precision of M may plausibly be assumed to vary in like manner and the quantity μ may well be replaced by a value that depends on n.

Thus we may put

$$K = \frac{C}{\sqrt{n}},$$

where C is an arbitrary constant.

But for $n = 1$, $\mu = M$, therefore $C = M$ and

$$\mu = \frac{M}{\sqrt{n}}.$$

Finally

$$M^2 = m^2 + \left(\frac{M}{\sqrt{n}}\right)^2$$

or

$$M^2\left(1 - \frac{1}{n}\right) = m^2$$

whence

$$M = \sqrt{\frac{v_1^2 + v_2^2 + \cdots + v_n^2}{n - 1}}.$$

This new value for M gives a true mean error calculated from the practical errors. It is evident that the calculation of m is hardly desirable when M can be obtained from the same data by using $n-1$ instead of n.

Mean error and average error.

The relation of M to D in a given system of probabilities is constant: in the system of probabilities generally assumed for measurements

$$\frac{M}{D} = 1.25331.$$

Since the relation $\frac{M}{D}$ is a constant one, it can at once be concluded that

$$D = \frac{v_1 + v_2 + \cdots + v_n}{\sqrt{n(n-1)}}.$$

Thus the average deviation, or average error, calculated in this way is practically a true value. There can hardly be any excuse for using the value d. The extra labor in calculating D is not worth considering. For example, if there are 25 measurements the divisor will be 24.5 instead of 25: with CRELLE's Rechentafel at hand the extra trouble is small. On the other hand, the average deviation thus formed is not a rough approximation but one so close that it is practically true.

This same consideration holds good for statistical researches, although it seems not to have always been taken into account.

Which of the two characteristic deviations D and M should be used? The quantity M calculated for the second powers of the variation is preferable to D calculated from the first powers or to any value calculated from powers higher than the second for the following reasons:

1. A change in the conditions of the measurement will change the values of V. If the conditions are more precise, the V's will be smaller, if less precise, larger. But for any particular method the V's will be grouped more closely around a certain value than they will be around that value in any other method whether more or less precise; this value is M. M is in this way an error specially probable in the particular set of measurements.

2. The probability for the occurrence of a set of errors $V_1, V_2, \cdots, V_n$ is the same as that for the occurrence of a set in which each $V = M$.

3. The fact that n is not infinite gives an uncertainty to D and M when they are calculated for the practical errors v. This uncertainty has been found by GAUSS[1] to be for D

[1] GAUSS, *Bestimmung der Genauigkeit der Beobachtungen*, Werke, IV 109.

$$\pm \frac{0.756}{\sqrt{n-1}}$$

and for M

$$\pm \frac{0.708}{\sqrt{n-1}}.$$

Thus M is more precise than D.

Probable error.

Still another representative error is in use. If the errors $v_1, v_2, \ldots, v_n$ are arranged in order of size, for example the smallest to the left and the largest to the right, then the one in the middle could be chosen as the characteristic variation. For an infinite number of measurements this would be what is called the "probable error." In an infinite number of measurements there would be just as many variations, or errors, bigger in size than the probable error as there would be those smaller; or if any one measurement were picked out by chance from the infinite series, the proper wager would be 1 to 1 that its error would be larger (or smaller) than the probable error. In an infinite number of measurements the relation holds

$$\frac{P}{M} = 0.67449.$$

Thus

$$P = 0.67449\, M$$

or closely

$$P = \frac{2}{3} M.$$

It is also true that

$$P = 0.84533\, D.$$

In a finite series of measurements the probable error cannot be picked out, but may be calculated from the mean error.

The fact of the almost exclusive use of the probable error by the other sciences makes it obligatory for psychology to use it in preference to the mean error or the average error, although in discussion of the results it is often favorable to use the mean error also.

II. Example of computation.

The character and purpose of these formulas may perhaps be made clearer by an illustrative example.

Let the quantity to be measured be the simple reaction-time to the click of a sounder by a release movement of the index finger, while a certain subject is in a condition of prepared and undistracted attention.

The latter stipulation is met by placing the subject in a quiet, comfortable, well-ventilated room, by making the experiments in small groups, separated by short intervals of rest, by giving a warning signal before each experiment and by the other usual precautions. The apparatus is so arranged that the stroke of the sounder makes the stimulus-record by a spark, a marker or otherwise; the key is so arranged that the slightest movement of the finger makes the reaction-record.

The records for Subject *A* are 251, 257, 265, 240, 219; pause; 231, 240, 242, 237, 245; pause; 241, 235, 243, 237, 239; pause; 243, 249, 239, 251, 235; pause; 254, 237, 263, 263, 237.

The record-blank will show the figures as indicated under the word *Record.*

Record.

251	+ 7.3	53.29		+ 5	25	4)1272
257	+ 13.3	176.89		−11	121	318
265	+ 21.3	453.69		+19	361	√ = 18
240	− 3.7	13.69		− 6	36	⅔ √ = 12
219	− 24.7	609.09	246	−27	729	
231	− 12.7	161.29		− 8	64	4)102
240	− 3.7	13.69		+ 1	1	26
242	− 1.7	2.89		+ 3	9	√ = 5
237	− 6.7	44.89		− 2	4	⅔ √ = 3
245	+ 1.3	1.69	239	+ 6	24	
241	− 2.7	7.29		+ 2	4	4)40
235	− 8.7	75.69		− 4	16	10
243	− 0.7	.49		+ 4	16	√ = 3
237	− 6.7	44.89		− 2	4	⅔ √ = 2
239	− 4.7	22.09	239	0	0	
243	− 0.7	.49		0	0	4)197
249	+ 5.3	28.09		+ 6	36	49
239	− 4.7	22.09		− 4	16	√ = 7
251	+ 7.3	53.29		+ 9	81	⅔ √ = 5
235	− 8.7	75.69	243	− 8	64	
254	+ 10.3	106.09		+ 3	9	4)689
237	− 6.7	44.89		−14	196	172
263	+ 19.3	372.49		+12	144	√ = 13
263	+ 19.3	372.49		+12	144	⅔ √ = 8
237	− 6.7	44.89	251	−14	196	
245	+104.7	24)2802.05				
−1.3	−104.2	116.75				
243.7	+ 0.5	√ 10.81				
		⅔ √ = 7.21				

Computation of the average.

As all measurements were taken under the same conditions, they should be averaged together directly. In the present case where the groups are equal in size the group averages might be obtained and then averaged with the same result; but where the groups are of unequal size this will not do.

The usual way of computing the average when the figures are large is to assume first an approximate figure. Cursory inspection shows that the average is about 245. By mental calculation we find that the individual measurements differ from this by + 6, + 12, etc., and we add or subtract directly as we proceed. The eye does not need to notice the first figure 2 as it is constant. Running over the figures 51, 57, etc., we note + 6, + 18, + 38, + 33, + 7, − 7, − 12, − 15, − 23, − 23, − 27, − 37, − 39, − 47, − 53, − 55, − 51, − 57, −51, − 61, − 52, − 60, − 42, − 24, − 32. That is, the sum of all the measurements exceeds 25 times 245 by − 32. Dividing the excess by the number of measurements we find that 245 must be corrected by $-32 \div 25 = -1.3$ to give the average. The average is thus 243.7. Computation with long columns of figures can thus be carried on with ease and accuracy. The addition may advantageously be performed by an adding machine or an oriental abacus. This latter is a simple device used by the Chinese and Japanese. After some practice with an abacus or an oriental counting frame interminable additions can be carried on with great accuracy and with very little mental labor.

Computation of the variations from the average.

The variations of the individual results from the average can generally be obtained mentally. The eye does not need to notice the first figure 2. The results are placed in the second column. It is frequently convenient to first fill in the signs in this column; thus all measurements larger than 243.7 will yield +, all smaller ones will yield —. Likewise the decimals may be filled in: .3 where there is a + and .7 where there is a —. When this has been done the computation may readily be carried out by subtracting 44 from the figure with a + and by subtracting from 43 for that with a —.

We next find the sum of the positive errors and the sum of the negative errors; they should be equal in size. The difference is found to be + 0.5 which is 25 times the amount by which 243.7 is smaller than the average; thus the average would be 243.72 if the second place were to be regarded.

Computation of the probable error.

The errors are now to be squared with the aid of a table of squares such as is to be found in every book of tables. The results are placed in the third column. The addition of this column may be aided by the use of an adding machine or an abacus. The sum of the squares is 2802.05. Dividing by 24 we get 116.75, of which the square root is 10.81. The probable error is ⅔ of this, or 7.21. Thus the average time is stated to be $243.7^\sigma \pm 7.21^\sigma$.

This means (*a*) that upon making other measurements under exactly the same circumstances we can expect the result of any one measurement to fall within the region bounded by $243.7^\sigma + 7.21^\sigma$ and $243.7^\sigma - 7.21^\sigma$ just as often as it will fall outside of it; (*b*) that, since all instrumental and external sources of variation were eliminated, the value 7.21^σ represents the internal uncertainty of Subject *A*'s mental process in the given case.

The great value of the characteristic errors as measures of mental activity seems to have been generally overlooked by psychologists. It has been in constant use in the Yale laboratory for a number of years and has furnished very important data, for example in the researches of GILBERT on school children[1] and of NADLER on alcoholism and other nervous troubles.[2]

Testing the average.

It is desirable to get some idea of how well the average represents the set of measurements. The choice of the average as a representative value rests upon the assumptions: (1) that in an infinite series of measurements there would be as many values larger than the average, as there were smaller; (2) that extreme values are less frequent than those grouped around the average. In the present set there are 9 values larger and 16 smaller than the average: if in further sets of measurements the case is found to be similar, the conclusion would be reached that even in an infinite number of measurements the disproportion would probably still prevail and that the average is not a very good representative value. In such a case it would be well to recompute the results, using the median as the representative value. The median is the middle value in the series according to size; in a set of 25 measurements it is the 13th from either end when the values are arranged in order of size. In the set we are computing the

[1] GILBERT, *Researches on the mental and physical development of school-children*, Stud. Yale Psych. Lab., 1894 II 43.

[2] NADLER, *Reaction-time in abnormal conditions of the nervous system*, Stud. Yale Psych. Lab., 1896 IV 1.

13th figure, or the middle value, is 240. But the 12th value is also 240; we therefore change 240 by a small amount. This amount is calculated as follows: There are 11 measurements smaller than 240 and 12 larger. We take the excess of the smaller over the larger, or -1, and divide it by twice the number of values identical with the middle value; this gives $\frac{-1}{2 \times 2} = -\frac{1}{4}$. Adding the result to the middle value, we have $240 - \frac{1}{4} = 239\frac{3}{4}$ or, since only the first decimal is considered, 239.8 as the median.

The fact that the median is smaller than the average indicates a grouping of the measurements around small values with a few extremely large ones. This is a very frequent case in psychological work; if it were not for the almost universal use of the average in the other sciences, it might be desirable to use the median as the regular representative value for much psychological work. In any case it is very desirable to use the median as well as the average whenever hypothesis (1) is not fulfilled. When it is fulfilled, the median and the average are the same.[1]

This lack of symmetry in the distribution of the results often indicates some one factor or some group of factors entering overpoweringly into the phenomenon measured. Thus, if the results from Subject *A* have been steadily symmetrical for several occasions and then show asymmetry, we would strongly suspect that—instrumental and bodily conditions remaining the same—some new mental factor had entered into his reaction; for example, his method of listening for the signal may have—perhaps unknown to himself—become quite different. The second hypothesis is not well supported in the above example. There are 8 or 9 values out of 25 that can be considered as very large or very small. This indicates that the phenomenon we are measuring is lacking in unity. By unity we mean that the phenomenon is composed of parts of constant characters in constant relations. Thus simple reaction may be usually assumed to consist mentally of perception and volition;[2] there are, however, undoubtedly very many other elements involved which act differently and irregularly in some cases. The prevalence of extreme values tells against the reliability of the average or any other representative value, and indicates the need of more careful study of the conditions of the experiment. Our example is quite typical of the usual results; it is undoubtedly true that there are unrecognized mental elements in simple reaction-time which await the discoverer.

[1] Literature and discussion on these points may be found in SCRIPTURE, *Mean values for direct measurements*, Stud. Yale Psych. Lab., 1894 II 1.

[2] SCRIPTURE, New Psychology, 152, London 1897.

Testing the law of distribution.

It is next desirable to have an indication of the law of distribution for the results. The *assumption* of the average as the *most probable* value in addition to the hypotheses (1) and (2) above leads to the well-known law of probability

$$y = \frac{h}{\sqrt{\pi}} \cdot \varepsilon^{-h^2x^2}.$$

According to this law the following relations would hold good for an infinite number of measurements :

(α) Average = median ;

(β) Mean error = 1¼ times average error ;

(γ) Probable error = median error.

(δ) Number of successions of the same sign (+ + or — —) before the errors in the order of occurrence = number of changes (+ — or — +).

(α) In the 25 measurements of our example we have already found that α cannot be considered to hold good.

(β) The average error can be readily calculated by adding the sums of the + and — errors already found, disregarding the sign. We get 208.9, and, dividing by $\sqrt{25 \times 24} = 24.5$ we have an average error of 8.5. The mean error 10.8 is 1.27 times the average error. This is very close to the relation 1.25 required by the ideal law of probability.

(γ) If we consider all the errors regardless of sign we find that there are 11 larger and 14 smaller than the probable error 7.2.

This would not be a bad indication if it were not for the fact that two of the 11 are 7.3, thus differing very little from 7.2 ; the relation is more nearly 9 larger to 14 smaller. The curve of probability is, therefore, apparently fairly—but only fairly—smooth and regular if we treat all the errors as positive. This gives us some confidence in its steadiness of the phenomenon under measurement.

(δ) There are 13 cases of succession (+ + or — —) and 11 of change (+ — or — +), which would be a favorable sign if the successions and changes were more evenly distributed.

The conclusion which we must draw from these tests is that the phenomenon we are measuring is fairly under control but probably contains more than one factor or close group of factors. This indicates the possibility and desirability of further investigation aiming at the analysis of "simple" reaction-time into factors more nearly elementary.

Search for systematic errors.

It is frequently possible to determine directly from the results some of the factors undergoing change. Since the measurements were made in somewhat independent groups, the average and the probable error for each group may be considered.

The averages are 246, 239, 239, 243, 251 (p. 116). Owing to the small number of measurements for each average no reliance can be placed on the tenths arising by division and these averages are therefore not carried beyond the unit's place. The deviations from the general average are + 2, — 5, — 5, — 1, + 7. This would indicate a systematic influence tending at first to shorten the reaction-time and afterwards to lengthen it as the experiments are made in succession. The early shortening is usually termed the "influence of practice," and the later lengthening the "effect of fatique."

The probable errors are 12, 3, 2, 5, 8 (p. 116) or, as percentages of the respective averages, 5%, 1%, 1%, 2%, 3%. These indicate that the subject was in the first group quite irregular, that in the second he became much more regular and remained so in the third, whereas he lost some regularity in the fourth and still more in the fifth. The early increase in regularity is also termed the "influence of practice" and the later loss in regularity the "effect of fatigue."

There are thus two sources of systematic error known as practice and fatigue. To investigate these there should have been no intervals of rest between groups of five and the whole set might have been longer. If the object of the present set, however, was to measure the reaction-time in a constant condition of practice and with no effect of fatigue, the grouping was not fully effective. To improve it the first group might be omitted if it appeared as a regular thing in all cases that the first group differed from the others. The last group might perhaps also be omitted. The intervals between the groups might possibly have been longer with good effect.

A search for systematic errors within a single group might be attempted by averaging all the first values, then all the second values, etc., as indicated in the computation on p. 122. The averages are 244, 244, 250, 246, 235. The probable errors are $\pm$ 6.1, $\pm$ 6.1, $\pm$ 8.8, $\pm$ 7.5, $\pm$ 6.5. There is first a lengthening and then a shortening of the average time, and first a decrease in regularity and then an increase. These are directly opposed to the course of the systematic errors mentioned above as "influence of practice" and "effect of fatigue." If this same peculiarity should appear in most of the other sets of measurements, a new source of systematic error would have to be looked for.

	v	v^2	M	P
251	+ 7	49		
231	− 13	169		
241	− 3	9		
243	− 1	1		
254	+ 10	100	9.1	6.1
244		328		
257	+ 13	169		
240	− 4	16		
235	− 9	81		
249	+ 5	25		
237	− 7	49	9.2	6.1
244		340		
265	+ 15	225		
242	− 8	64		
243	− 7	49		
239	− 11	121		
263	+ 13	169		
250		628	11.5	8.8
240	− 6	36		
237	− 9	81		
237	− 9	81		
251	+ 5	25		
263	+ 17	289		
246		512	11.3	7.5
219	− 16	256		
245	+ 10	100		
239	+ 4	16		
235	0	0		
237	+ 2	4		
235		376	9.7	6.5

Reliability of the average.

Assuming that the measurements followed the ideal law of probability we have the average as the most probable value. The assumption excludes asymmetry and systematic errors.

We have already said that, on making another single measurement under the same conditions, its value ought to be just as likely to fall within the limits 243.7 ± 7.21 as to fall outside of them.

But suppose we make another series of measurements under the same circumstances; their average ought to be more likely to fall within the limits than outside them. It is found that this likelihood increases as

the square root of the number of measurements used for the average. Thus it is as likely as not that one single measurement will fall within the limits 243.7 ± 7.21 but it is just as likely as not that a similar set of 25 measurements will fall within the limits $243.7 \pm \frac{7.21}{\sqrt{25}} = 243.7 \pm 1.44$.

An average derived from 25 results should have a probable error one-fifth as large as one of its single measurements. We can therefore say of the average 243.7 that, if the probable error for a single measurement be taken as unity, its probable error should be one fifth of that amount. The probable error for a single measurement in this example is 7.21 and that for the average will be 1.44.

The average and its probable error are therefore stated to be 243.7 ± 1.44.

This probable error has quite a different meaning from that discussed on page 118. The other one served as an indication of the unity of the process involved; this one shows how much value is to be attached to the average as a numerical figure. If the measurements had been stopped at the fifteenth, the average and the original probable error might possibly have been found to be exactly the same, but the probable error for the average must be larger because the average was derived from a smaller number of measurements and the original probable error would be divided by $\sqrt{15}$ instead of $\sqrt{25}$.

For psychological work it is nearly always necessary to give the probable error for a single measurement and the probable error for the series. The use of the latter may readily be illustrated by requiring a result precise to a certain percentage. The average 243.7 is precise to $\frac{1.44}{243.7}$ or 0.7%. If the required percentage had been 2%, an unnecessary number of experiments was taken; if it had been 1%, there were not enough.

To illustrate let us consider the following problems: Subject *A* is tested for the results of a change in one of the factors of his mental condition. In one set of experiments *a* all the conditions are exactly as in the example just discussed; in another set *b* he receives the instructions: "Make an intense effort to act quickly." The sets are repeated a number of times in the order *a b b a b a a b*, etc. Let us suppose that the results when averaged show for *a* 45 measurements with an average of 243.6 and a probable error of 8.1 and for *b* 57 measurements with an average of 212.2 and a probable error of 11.2. These results would show that the extra effort of will shortened the reaction-time and made it less regular. The two results do not, however, possess the same reliability.

The probable error for the first average is $\frac{8.1}{\sqrt{45}} = 1.2$ or 0.49% and that for the second $\frac{11.2}{\sqrt{58}} = 1.5$ or 0.77%. The average for set b is, therefore, less reliable than that from a and the number of experiments should have been still further increased.

The probable error of the average may conveniently be indicated by p, to distinguish it from the immediate probable error P (p. 115).

STUDIES

FROM THE

Yale Psychological Laboratory

EDITED BY

EDWARD W. SCRIPTURE, PH.D.

Director of the Psychological Laboratory.

1901

VOL. IX.

YALE UNIVERSITY

NEW HAVEN, CONN.

PRESS OF

THE NEW ERA PRINTING COMPANY

LANCASTER, PA.

CONTENTS

RESEARCHES ON THE RHYTHM OF SPEECH

BY

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I. Historical.

Brücke[1] employed two methods in investigating the time relations of syllables and feet in verse.

In the first method the subject of the experiment beat time with the finger upon a key, in unison with the rhythm of the scanning of the particular verse that was read by him. The key was connected with a marker, which was so arranged that the deflections were traced upon a recording drum. The distances between the checks in the record line were regarded as representative of the duration of time between the successive points of emphasis.

In the second method a recording lever was attached to the lower lip or to the teeth of the lower jaw, or, though rarely, to the corresponding parts of the upper jaw. A series of monosyllables, consisting of such words as *ba*, *bam*, *pap*, etc., was given to the subject to scan with the utmost regularity in accordance with predetermined types of meter.

The general conclusions indicated that the intervals between the accented syllables of poetry are exactly equal irrespective of the quality of the material which constitutes the intervals. Whenever the number of unemphatic syllables which occur between two accented syllables exceeds the average, they are uttered with a rapidity sufficient to make the intervals equal as to time; and whenever, on the other hand, the number is deficient, the loss is made up either by prolonging the syllables or by introducing pauses.

Brücke's investigation was concerned with a purely mechanical or artificial method of scanning, different from the natural, free, or artistic reading. It is hardly justifiable to apply his conclusions to ordinary spoken verse in support of the principle of equality of feet.[2] The free rhythmical flow is the expression of impulses, unconsciously operative, that are in the mind of the poet in writing verse.[3] Mechanical scansion is the

[1] Brücke, Physiologische Grundlagen der neuhochdeutschen Verskunst, Wien 1871.

[2] Paul, Grundzüge d. germanischen Philologie, II (1) 909, Strasburg 1893.

[3] Poe, The Rationale of Verse, Works VI 84, 92, Chicago 1895.

expression of impulses consciously striving to produce perfect uniformity, for the sake of a certain pleasure which is derived from the satisfaction of expectant attention. In so far as it is perfectly mechanical, it disregards the logic and sense of the word, for the fuller gratification of these impulses which are relatively incapable of being intellectualized and which subserve but a limited function.

BRÜCKE's conclusions are accurate only within wide limits owing to the inaccuracy of the apparatus of that date. The measurements were recorded on a crude form of the kymograph, the rate of rotation of which could not be increased sufficiently to register[1] the minute differences which inevitably existed between the individual measurements.

In reference to BRÜCKE's second method, we have to consider that the concatenation of syllables in the particular series that was scanned, was arbitrary and artificial. The syllables were uniformly monosyllabic, and of such a character as to require relatively equal strain on the organs of speech in uttering them. A combination of difficult and easy syllables, physiologically considered, would probably have yielded different results.

The experiments of HURST and MCKAY,[2] with more accurate apparatus, were measurements of the intervals between the beats of the finger made in unison with scanned verse. The method was, in the main, like that of BRÜCKE.

The following conclusions were reached: (1) the feet of a given verse are equal in length; (2) dactyls and trochees are shorter, respectively, than anapæsts and iambics; (3) a radical difference exists between the anapæst and the dactyl, the length of the syllables in the former being of an "ascending," in the latter of a "descending," order; (4) there is a lack of a fixed proportion between syllables—the emphatic, however, being longer than the unemphatic.

The postulates which underlie all similar attempts at measuring the duration and equality of poetical feet, are essentially two: (1) the rhythm of the scanned verse agrees with the rhythm of the taps of the finger, and (2) neither exercises an influence upon the other and the rhythm of the finger does not tend to regulate the rhythm of the scansion. Neither of these assumptions is legitimate. In the first place, the two types of motor innervation do not exactly correspond. This has been proved by the experiments of MIYAKE, at the Yale laboratory, in which the subject beat upon an electrical key with the finger as the sounds *ma*, *pa*,

[1] MEUMANN, *Untersuchungen zur Psychologie und Aesthetik des Rhythmus*, Philos. Stud., 1894 X 418.

[2] HURST AND MCKAY, *Experiments on time relations of poetical meters*, Univ. Toronto Stud. (Psychol. Series), 1900 157.

etc., were spoken into a voice key. The beats of the finger preceded the sounds. A similar result has beeen obtained in the present series of experiments. The subject was required to beat time on the table upon which the phonograph rested in time with the rhythm of the accents of several verses of faultless meter, which were mechanically scanned. The finger beats were transmitted to the phonograph cylinder through the frame of the phonograph. The results showed discrepancies between the two beats, the beat of the finger generally being anterior to the metrical accents. In the second place, the experiments, to which reference was last made, indicate that the movements of the finger serve as regulators of the movements of the larynx. The subject was directed to scan as regularly as possible both with and without the beats of the finger. The result, as may be seen by turning to the E. H. T. records (Tables LXII. and LXXV. below), was that the regularity in the former instance was more than trebled. This explains the high degree of regularity obtained in the measurements of the two preceding series of researches.

Král and Mareš[1] made use of a more direct method of measuring vocal sounds. The apparatus employed was a telephone receiver, in electrical contact with the nerve of a muscle of a frog's leg, the contractions of which were registered upon the smoked paper of a kymograph by means of a recording arm. The sounds spoken into the receiver produced electrical vibrations that stimulated the nerve and caused the muscle to contract.

The experiments included two kinds of measurements: (1) the length of syllables, long and short, uttered separately; and (2) the measures of scanned verse. The language spoken was Bohemian.

Four general results were obtained in this investigation: (1) long vowels and diphthongs, though generally longer than short vowels, were often of the same length; (2) neither of these had absolute time-values, even for the same person; (3) consonants required a very short time for their utterance, the addition of consonants to syllables not being attended by a proportionate increase in the duration of the syllables, and some additions of consonants tending to shorten rather than lengthen the syllables; (4) even in attempting to scan verse so as to exactly coördinate the time of the measures, an equality of time-values was not obtainable, whether the scansion was according to the time theory or the emphasis theory of meter.

While syllables should be measured, not as independent entities but as interrelated elements of a complex group of syllables—the conditions

[1] Král a Mareš, *Trvání hlásek a slabik dle objectivné míry*, Listy Filologické, 1893 XX 257.

under which they occur in speech—these conclusions are important as affecting the comparison of like syllables under like conditions. The conclusion with reference to the time-value of poetical measures, it is interesting to notice, is at variance with the conclusions of the two former experimenters.

MEYER,[1] in a series of experiments the publication of which appears never to have been completed, has measured the syllables and feet of spoken verse. An electric tuning fork vibrating 100 times in a second recorded the time line on a smoked drum. A MAREY tambour registered the breath pressure from the mouth. Two trumpet-shaped speaking tubes of paper were attached to the tambour by means of a short glass or metal tube. One of the speaking tubes was made to conveniently fit the mouth cavity; the other, the nose. The tubes could be used singly or in combination. The result of speaking into the tube was to transmit to the rubber diaphragm of the tambour a series of waves corresponding to the sounds and silences of the utterances. The fluctuation of the diaphragm were registered by an amplifying straw lever. Thus the duration of the successive explosions of the stream of outflowing air could be measured. The successive puffs of air were hypothetically assumed to extend over the same times as the successive sounds.

A further development of the apparatus consisted in connecting a tapping instrument to a recording lever whose point was adjusted immediately under the point of the tambour.

The method of taking the records consisted in repeating monosyllables singly or as a series of words, at the same time beating with the finger upon the tapping instrument in time with the articulation of the syllable at the moment of greatest emphasis ("Arsengipfel"). The subject was thoroughly trained in coordinating the time of tapping with the time of uttering the words. The attempt was made to make that point of the syllable which represented the climax of the energy exerted by the vocal organs exactly correspond with the tapping time. This was assumed to be possible, for since both processes start from the same center the maximum points of force of the two innervations should occur synchronously. Thus the tapping mark upon the drum, regardless of where it occurred in the course of the sound, represented the point at which the energy of the syllable was the highest.

The object of the investigation was twofold: (1) to determine the point of highest energy of single syllables beginning with different consonants, such as *pha*, *fha*, *ha*, *za*, *fma*, etc.; (2) to investigate the meter of German verse.

[1] MEYER, *Beiträge zur deutschen Metrik*, Neuere Sprachen, 1898 VI 1–37; 122–140.

The author makes the following general deductions: (1) the point of energy occurs in the initial consonant or consonants somewhere shortly before the beginning of the vowel, varying somewhat according to the difficulty of articulating the consonants[1]; (2) the moment of the least, and not the moment of the highest, energy of speech determines the boundaries of the foot[2]; (3) whenever the beginning of the arsis (part of syllable preceding point of highest energy) coincides with the beginning of a syllable, and the end of the thesis (part following the point of highest energy) coincides with the end of the syllable, the boundaries of the foot coincide with the boundaries of the syllables[2]; and (4) whenever unemphatic syllables uniformly coincide with the arsis or the thesis of an accented syllable, the boundaries of the foot of verse and prose ("Sprechtakt") become identical.[2]

MEYER states[3] that the investigation was subject to the following sources of error, which were incapable of elimination: (1) latent time of the apparatus ("Registrirverzögerung"), some loss in the time of transmission, and in the quantity of the energy to be recorded in the passage from the receiving to the recording points being inevitable; (2) errors of measurement ("Wahrnehmungsverzögerung") owing to the limited capacity of the visual organ to discriminate small differences.

The following considerations, as affecting MEYER's experiments, may be noted: (1) The measurements are essentially measurements of breathing. The air waves may or may not precisely coincide with the sounds. (2) An exact simultaneity in the occurrences within the central organs of the highest points of energy for the innervations of the vocal muscles and the muscles of the hand may perhaps be assumed. The additional assumption is made that the registering of the impressions are also synchronous. Since, however, the media for transmitting the impressions are not the same (the air and the hand), this may be unjustified. Moreover, it has been proved that no muscular movements are capable of exact coordination. Contrary to the supposition (although the movements of the hand are not reactions to the movements of the organs of speech) that the movements of the hand and vocal organs are not subordinate but coordinate, the movements of the hand may serve as a regulative concomitant of the movements of the vocal organs. For this reason the results may not apply to the rhythm of free declamation.

[1] MEYER, *Beiträge zur deutschen Metrik*, Neuere Sprachen, 1898 VI 134.

[2] MEYER, as before, 138.

[3] MEYER, as before, 26–30.

TRIPLETT and SANFORD,[1] using apparatus similar to that of BRÜCKE, HURST and MCKAY and MEYER, have measured eleven nursery rhymes of long, common and short meter, scanned by themselves eight or ten times with great regularity, and have studied without apparatus, the patterns adopted. A finger record of taps, a vocal record representing breath puffs, and a time line, made by an interrupter checking off tenths of a second, were traced on the kymograph. The voice record was used for comparing the exactness of the correspondence between the finger taps and the expiratory stresses. The intervals between the finger taps were measured.

The following conclusions were reached: (1) If the pause intervals between the larger rhythmic units are excluded, the measures of scanned nursery rhymes are approximately uniform. (2) There is a tendency to accelerate the speed of scanning the successive measures in the verse as well as the successive verses themselves. (3) No characteristic differences of speed are found between dactylic and trochaic, anapestic and iambic measures. This is contrary to the results of previous experimenters. (4) The most frequent patterns of verse have a characteristic movement, due partly to the distribution of the pauses and the tendency to increase the speed.

This cursory review of previously existing methods of experimentally investigating the time relations in speech illustrates and enforces the necessity of improved methods of speech investigation. Two types of methods may be used. The former is largely the method which has prevailed up to the present time. The sounds of spoken language have been measured by means of finger beats, currents of air and non-reproducible sound vibrations. A more direct method consists in measuring directly the sounds recorded in, and reproduced by a talking machine. This method has the following advantages:

1. The instantaneous action of the recording stylus of the modern talking machines practically eliminates the errors of the "latent time" of the apparatus and the recording of superfluous and irrelevant movements.

2. It is the only method by which the accuracy of the recorded impressions can be completely verified, since it alone affords the means of reproducing the sounds that have been recorded. The accuracy of the impressions recorded upon instruments of the non-reproducing kind, must always remain more or less conjectural.

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[1] TRIPLETT AND SANFORD, *Studies of rhythm and meter*, Am. Jour. Psych., 1901 XII 361.

II. Apparatus.

The experiments now to be described were begun in the early summer of 1900 and carried out during the academic year 1900–1901.

The machine used in making records was a Lioret phonograph, resembling the graphophone in its construction. The rate of speed was accurately recorded.

For reproducing purposes an Edison phonograph was used. A gear wheel with twenty-four teeth having the spaces filled with vulcanized rubber was placed on the axle of the phonograph. A metal spring resting upon the wheel made contact with each tooth. Every revolution of the contact wheel made twenty-four closures of the circuit. An Ewald magnetic counter placed in the circuit indicated the number of teeth passing under the end of the spring.

A telegraph key was placed on each side of the phonograph. By pressing the knob of the key on the left side, the current was sent through the counter; by pressing the key on the right side the circuit was interrupted. In this way the number of contacts between the movements of the two keys was obtained. A knowledge of the rate of revolution of the phonograph cylinder when the record was taken made it possible to translate the number of contacts into seconds.

The limit of the greatest possible speed capable of being utilized was determined by two factors: (1) the degree of dexterity developed by the experimenter in reacting upon the telegraph key to the given impressions; and (2) the limit of accuracy of the magnetic counter. Beyond a certain rate of speed the counter failed to register all the contacts. In none of my measurements was this limit even approached. The highest number of contacts per minute required in the measurements was 2,880. The instrument was capable of registering a much higher number of contacts. Within the limits of speed, however, the accuracy of the measurements depended upon the maintenance of a fairly constant relation between the strength of the electrical current and the tension of the spring of the armature of the counter magnet. The accuracy of the chronoscope was verified by comparing the gross measurement of a large interval (*e. g.*, a verse or a stanza) with the aggregate of all the smaller intervals composing it (the measures and pauses); and by comparing the time required to measure off a stanza with the time required to record it, which had been ascertained by the watch.

The limit of the slowest speed was dependent upon discriminative audibility. At a certain speed the sounds lose their articulate character.

Except for very special reasons, the speed used in measuring corresponded to the speed used in taking the records. This varied from 80 to 120 revolutions per minute. By maintaining the recording speed the records were heard, in respect to speed and pitch, as they were made. The measurements which were made before the experimenter developed a high degree of proficiency were rejected

The method of measuring consisted in first listening, through the ear tubes, to the record upon the cylinder and choosing the intervals to be measured. The intervals were then listened to repeatedly, and each one was measured from four to five times by pressing one key at the beginning and the other at the end. The average of the different measurements was recorded as the length of the interval. The successive measurements of the same interval differed only very slightly, and were frequently identical; the measurements were thus quite accurate.

In making the phonograph records upon which this research is based, the procedure was guided by the following considerations:

1. All mechanical distractions and impediments, such as noises, strain of the muscles of the body or head occasioned by a forced standing or sitting posture, interference with the easy movements of the lips, fatigue, etc., of the subjects, were eliminated as far as possible.

2. The material contributed for study should come from subjects without bias or preconception.

3. The subjects were uninstructed in respect to the purposes of the experiment, except where the nature of the experiment required otherwise, in order to minimize the elements of unnaturalness and intentional change of the manner of speaking.

4. The subjects were of varied characters, with different languages, from different countries, of different stages and walks of life (elementary and high school pupils, academic and graduate students, professors, poets, orators, musicians, etc.).

5. The character of each record was made a true copy of the natural characteristics of the mode of delivery of the subject, so as to be typically his own, no matter what the idiosyncrasies.

By requiring the utterance to be natural the research could assume the rôle of being an investigation into the rhythmic peculiarities of actual speech. In a few cases, however, it seemed advisable to omit offering any instructions and the matter was left entirely to the taste of the reciter. Moreover, in proffering suggestions due care was taken lest the advice should tend to make the subject too self-conscious. Cases of mechanical scansion were also included for the sake of comparison and completeness of scope.

III. The centroid.

1. *Concept of the centroid.*—A portion of speech such as "the cities are full of pride," is physically a vibratory movement of a complicated form of which various portions can be assigned in succession to the sounds ð, *ə*, *s*, *i*, *t*, *i*, *z*, etc., with more or less abrupt changes (glides) between them. The series of sounds represents: (1) a continuous succession of vocal movements representing work, (2) a continuous vibration of an air particle, (3) a continuous succession of sensations. Both the speaker and the hearer feel the recurrence of points in the succession at which the impression reaches a maximum. These maxima are felt to have their positions determined not only by the actual maxima of vocal effort and of acoustic impression but also by the preceding sounds and by the anticipation of following ones. Owing to these circumstances the maxima as felt may differ more or less from the actual vocal or acoustic maxima. Such a maximum of effect may be called a 'centroid.'"

The English poetical line may be considered as a certain quantity of speech-sound distributed so as to produce an effect equivalent to that of a certain number of points of emphasis at definite intervals. The location of a point of emphasis is determined by the strength of the sounds at and around it. It is like the centroid of a system of forces or the center of gravity of a body in being the point at which we can consider all the forces to be concentrated and yet have the same effect. The centroid is not a syllable or a single sound but a point in the course of a sound.[1]

In the stream of vocal sounds the centroid lies in the syllable that relative to adjacent syllables, contains the maximum amount of force or loudness. The syllable containing the centroid may be called the "centroid syllable." It has been called the accented, emphatic, stressed, long or loud syllable.

Every syllable in a verse or sentence may contain a centroid. Whether it is actually to become a centroid syllable when the words are read, will depend upon its logical function and the circumstances of the utterance.

When the component sounds of speech are considered separately, the centroid represents the maximum of energy of a syllable. This point may, theoretically, either fall midway in the syllable, or precede or follow the middle point. According to Brücke,[2] basing his deduction upon the

[1] Scripture, *Researches in experimental phonetics* (*first series*), Stud. Yale Psych. Lab., 1899 VII 101.

Scripture, Elements of Experimental Phonetics, New York 1901.

[2] Brcüke, Physiologische Grundlagen der neuhochdeutschen Verskunst, Wien 1871.

measurement of scanned verse, when the vowel of the emphatic syllable is short, the maximum of energy occurs at the end of the vowel or just before the following consonant; when the vowel is long and not immediately followed by a consonant, it lies somewhere in the course of the vowel; and when the vowel is long, but does not terminate the syllable, the point lies very near the boundary between the vowel and the consonant. According to MEYER[1] the point of highest energy in syllables consisting of a vowel with an initial consonant lies in the early part of the syllable, shortly before the beginning of the vowel sound.

The physical factors that locate the centroid are increase of amplitude, increase of length and change in pitch.

Typical cases would be $c = a_{0.7} + l_{0.2} + p_{0.1}$, or $c = a_{0.4} + l_{0.2} + p_{0.4}$, or $c = a_{0.3} + l_{0.4} + p_{0.3}$, etc., where c indicates the strength of a given centroid, a, l and p the factors of amplitude, length and period, and the subscripts the share contributed by each.

2. *Kinds of centroids.*—Centroids admit of distinctions of kind only from the psychological point of view. We may use the term "phrase centroid" to denote the major or chief centroid of a given phrase. Each phrase should contain one chief centroid, one dominant idea, one specially important word.

According to the purposes of thought and feeling subserved by the centroid, we obtain a threefold division: rhythmic, pronunciatory and logical.[2] The function of the first is to mark off the different measures of verse; of the second, to emphasize the root syllables of words; of the third, to single out prominent words for special emphasis.

SUNDÉN[3] distinguishes two kinds of accent in Swedish: (1) Word accent, or the stress belonging to a separate word, constituting the word when of two or more syllables a sound unity. This is subdivided into (*a*) compound, or such accent as belongs to polysyllabic words having a *grave* accent ("*gravis*" = accent with low tone) on one syllable and a *strong* subsidiary accent ("*levis*") on the other, *e. g.*, in *Anna*, *konung* and *månsken*; and (*b*) simple, or the stress belonging to a monosyllabic word with *acute* accent ("*akut*" = accent with high tone), and a polysyllabic word with a *weak* subsidiary accent, *e. g.*, in *bok*, *böcker* and *böckerna* (*akut* on first syllable). (2) Phrase or rhetorical ("*oratorisk*") accent, or the stress of a word in its relation to other words.

Physically there are no distinctions of kind among centroids. There are no physical signs in the vibrations upon the phonograph cylinder

[1] MEYER, *Beiträge zur deutschen Metrik*, Neuere Sprachen, 1898 VI 123.

[2] LANIER, The Science of English Verse, Chap. IV., New York 1880.

[3] SUNDÉN, Svensk Språklära, 15, Stockholm 1895.

whereby the logical varieties can be distinguished. The physical process is a continuity, admitting of distinctions of degree, the general characteristics of which are shared by all centroids.

That we are justified in considering all kinds of emphatic syllables as centroid syllables, also appears from a consideration of the centroid as a process in consciousness. The centroid is an impression which arouses the sense of hearing to a certain pitch of intensity for a certain length of time. It is in respect of intensity that consciousness distinguishes it from other auditory sensations of the same nature. The strong sensations, irrespective of logical distinctions, are designated centroid syllables, the faint sensations, non-centroid ones.

Hence, as an objective physical occurrence, measureable in amplitude, period and length, and as an event in consciousness, capable of being roughly estimated in respect to intensity, any emphatic syllable may be included in the generic term of centroid syllable.

The psychological differences in the centroid may be accompanied by correlative differences in tone-color or timbre of voice.

3. *Method of locating the centroid.*—Two methods are available: (1) We may measure the tracings of sound curves of the talking machines. The centroid will be located at that point along the time-axis of a syllable where the influences of amplitude, length and pitch are the greatest. (2) By listening to a phonograph record, and comparing the consecutive syllables in respect to the intensity of the auditory sensation which they arouse, we may classify the syllables as weak and strong. This purely psychological method lies closest to nature, because speech centroids, fundamentally considered, are mental quantities.

The psychological method, according to which the centroids have been located in the present experiment, has been attended with some difficulty. It has sometimes, though rarely, been difficult to determine which of two, or whether any one of two, consecutive syllables is a centroid. Appeal has frequently been made to other persons. Where doubtful cases exist, the fact is indicated in the records by placing a ' directly after the centroid.

4. *The elements of the centroid.*—The centroid in speech is the result of a number of complex mental forces which constitute an original and inseparable synthesis, and which are the indispensable conditions of the life of the centroid. These elements, constituting the fundamental framework of the centroid, are capable of isolation by processes of analysis only; in any concrete case they constitute an irreducible complexity.

This complexity of elements is threefold—including the factor of time, factor of force, and factor of pitch. Every sound that is uttered is con-

ditioned on these three fundamental processes. Every sound is concrete—of a particular pitch, intensity and duration.

Furthermore, in order to excite the organ of hearing and give rise to a percept, every sound must attain a certain intensity, persist for a certain time, and fall within the limits of the psychological scale of pitch. Hence, both physically and psychologically considered, these three elements form the ultimate elments of the centroid.

5. *The essence of the centroid.*—The question now arises, which, or how many, of these factors constitute the essence of a centroid?

(*a*) Historical summary.—Several answers, somewhat obscure and inconsistent, have been given to the above questions. To enter into a detailed discussion of these would be out of place. The distinctions that have been introduced have turned either upon differences as existing between different languages or as existing within a given language.

According to SWEET[1] and POE[2] accent is practically non-existent in French poetry.

Accent in the case of ancient Latin[3] and Greek,[4] as well as in ancient and modern Sanscrit[5] and Persian, has been described in terms of pitch. The nearest modern approaches to this species of emphasis are the Swedish and Norwegian languages (*akut accent*), the verse of which, however, is governed by different laws. The Scotch emphatic syllable, on the other hand, has been characterized as having a "grave tone,"[6]—that is to say, as lacking almost entirely in pitch modification. Theoretically Scotch would afford an instance of a monotone speech. SUNDÉN[7] states that grave accent (*gravis*) is peculiar to Swedish and Norwegian; and elsewhere,[8] that in Swedish the elements of pitch (melodic accent, "*tonhöjd*") and loudness (expiratory or dynamic accent, *tonvikt*") coincide, and that an accented[9] syllable (*akut* or *gravis*) is always long.

The property of duration is said to contain the essence of the centroid in Latin, Greek, Arabic, Persian and Sanskrit, by MAYOR[10] and ELLIS.[11]

1 SWEET, Primer of Phonetics, 45–97, Oxford 1890.

2 POE, The Rationale of Verse, Works, VI 100, Chicago 1895.

3 ELLIS, The Quantitative Pronunciation of Latin, 21, London 1874.

4 SYMONDS, Sketches and Studies in Southern Europe, II 328, New York 1880. CLARK, Manual of Linguistics, 155

5 CLARK, Manual of Linguistics, 155.

6 GUEST, History of English Rhythms, II, London 1882.

7 SUNDÉN, Svensk Språklära, 15, Stockholm 1895.

8 As above, 14, 17.

9 SUNDÉN, Kort Öfversigt af Svenska Vitterhetens Historia, 20, Stockholm 1885.

10 MAYOR, Chapters on English Metre, London 1886.

11 ELLIS, The Quantitative Pronunciation of Latin, 21, London 1874.

In the following two groups, emphasis is resolved into terms of loudness or expiratory stress: Modern Greek, Italian, Spanish, German and English;[1] and old Italic, Keltic, Teutonic and Lithuanian.[2]

Just how far these distinctions are valid, it is not our purpose to discuss. This remains as a problem for future investigation.

We have reserved the theories of English accent for separate consideration. It is in this province that our chief concern lies.

It is difficult to classify, or bring into any sort of unity, the different utterances on the nature of the centroid in English verse, so indefinite and conflicting are the statements—statements that have been characterized as more pitiable nonsense than has ever been written on any other subject under the sun.[3] If we divorce the problem now under consideration from the connected problem of the quantitative character of English verse, which is reserved for discussion in the following section, the difficulty is increased. We are here concerned with the properties only of the centroid as such, and not of the centroid as a member of a succession of centroids.

The most prominent feature in the discussions of English prosody is the widespread assumption that the centroid in English is essentially a phenomenon of loudness, force, or stress. This position is maintained by ABBOTT and SEELEY,[4] MAYOR,[5] WALLIS,[6] GUEST,[6] GURNEY,[7] ELLIS,[8] LANIER[9] and others. A somewhat modified view is to the effect that there is a natural, although not an essential, connection between increase of force and increase of duration.[10] According to this view quantity is secondary: it is the variable element, while loudness is the stable element. ARNOLD[11] goes perhaps a step further in affirming that in English poetry, "accented syllables are long, and unaccented short." POE[12] holds that our starting point in the conception of verse is quantity or length; we begin with the long syllable, and the long syllable is the encumbered or emphatic one. JOHNSON,[13] the litterateur, transferred the conditions of classical accent to

[1] MAYOR, Chapters on English Metre, London 1886.

[2] CLARK, Manual of Linguistics, 155.

[3] POE, The Rationale of Verse, Works, VI 59, Chicago 1895.

[4] ABBOTT AND SEELEY, English Lessons for English People, 153–154, Boston 1880.

[5] MAYOR, Chapters on English Metre, III, London 1886.

[6] GUEST, A History of English Rhythms. IV, London 1882.

[7] GURNEY, The Power of Sound, 429, 433, 437, London 1880.

[8] ELLIS, The Quantitative Pronunciation of Latin, II, London 1874.

[9] LANIER, The Science of English Verse, I and II, New York 1880.

[10] SEELEY AND ABBOT, GUEST, MAYOR, ELLIS, SYMONDS.

[11] ARNOLD, Manual of English Literature.

[12] POE, The Rationale of Verse, Works, VI 59, 79 88, Chicago 1895.

[13] GUEST, as above; CORSON, A Primer of English Verse, IV, Boston 1893.

English verse: the emphatic syllable is invariably long. LOTZE[1] ventures the suggestion, without limiting its applicability to any one language, that the emphatic syllable always requires a longer "Sprechzeit."

MITFORD,[2] a musician, held that "sharpness" or pitch constituted the essence of the accented syllable; and SCHMIDT,[3] that there is an almost inseparable connection between pitch and intensity in English. Finally, SWEET[4] asserts a natural, though not a necessary connection between all the three elements.

(*b*) Experimental solution. (1) Method.—The method employed in determining the essence of the centroid was as follows: Selecting the centroids which had been previously located, of any given record, the stylus of the recorder was made to pass over each syllable a score or so of times, with varying rates of rotation of the cylinder. By thus listening attentively to the same syllable under varying conditions, it was possible to determine what element, or elements, were predominant in the sense-impression.

The results were indicated by placing appropriate symbols above the syllables. The next step consisted in verifying, by all available means, the results of mental discrimination. In respect to duration, the method employed was to measure the length of the syllables marked duration centroids; in respect of pitch, the syllables marked pitch centroids were compared with the pitch of adjacent syllables by aid of the piano. The element of duration and pitch could thus be estimated with great precision as regards their physical aspects. Viewed phychologically, however, that is to say, from the point of view of their importance as elements of intensive states of consciousness, a value could be assigned to each only when compared with other points of emphasis, by way of contrast, similarity, etc. Finally, the estimate of loudness reposes upon the immediate testimony of consciousness. These considerations, however, do not effect the certitude of the determinations with reference to the mere differentiation of the different factors. It is only as respects the degree of loudness that the judgments may be questioned.

Altogether 513 centroids, selected from eight different records were studied, namely, those of W. L. P., J. W. R., G. A. A., C. O. S., A. D. B., C. O., W. C. and W. W. The results are given in the two following tables.

[1] LOTZE, Geschichte des Aesthetik, 301.

[2] GUEST IV, and CORSON IV, as before.

[3] SCHMIDT, Introduction to the Rhythmic and Metric of the Classical Languages, 17, Boston 1878.

[4] SWEET, Primer of Phonetics, 67, Oxford 1890.

TABLE I.

Centroid elements.

Principle of synthesis.

	1	2	3	· 1	· 2	· 3	● · 1	● · 2	● · 3	●1	●2	●3	1	2	· 3	●1	●2	●3	● 1	● 2	● 3
n	250	35	3	5	10	20		9	7		14	1	3	0	2	1	4	2		4	1
t			288			41			16			15			11			7			5

n, number of centroids.

t, aggregate number of centroids per group.

1, 2, 3, degrees of centroids.

Number of centroids in entire series of groups, 383.

TABLE II.

Centroid elements.

Principle of substitution.

	: 1	2	·	●1	●2	· 3	△ 1	2	3
n	15	30	14	28	20	7	11	5	
t			59			55			16

n = number of centroids.

t = aggregate number of centroids per group.

1, 2, 3, degrees of centroids.

Number of centroids in entire series of groups, 130.

(2) Explanation of symbols.—The following symbols are designed to represent the properties, or property, which enter into the make up of the centroid.

The open circle (○) denotes a relative equality of pitch, loudness and duration emphasis. Ideally this sign should signify that the share contributed by anyone of these elements is equal to that of any one of the others. The equality, however, is only relative. In English and perchance in other modern languages, the element which has the greatest claim to supremacy is that of loudness. Hence the open circle is representative of a synthesis of three elements, in which the element of loudness is slightly predominant.

In the dot circle (⊙), or the circle with the dot in the center, the factor of loudness distinctly predominates over the factors of duration and pitch; in the closed circle (●) the element of duration preponderates; and in the inverted triangle the pitch factor overbalances the other two. Of the latter sort two alternatives are possible. The pitch emphasis may be due, in both cases by way of contrast, to a raising (△) or lowering (▽) of the pitch. The latter is the rarer of the two, as will be seen by a study of the records. Theoretically, either is an extreme departure from the medium pitch of any given record, and tends to an increase of emphasis.

The superposition of one symbol upon another signifies that the symbol which is placed underneath contributes the main part of the total effect. The upper sign indicates the element next in importance. The third element, of course, is subordinate to the element of the uppermost sign.

In each of these cases one element is predominant; while the other two are subordinate to this element, they may be equal to each other. The weaker elements are always present as the substratum of the centroid. They are the conditions of its very possibility as a vocal phenomenon. The abolition of any one signifies the abolition of the process of vocalization.

The effect of these several amalgamations, as well as the relations which they sustain to one another in respect to frequency and stress, will be seen in what follows.

6. *Degrees of the centroid.*—The subscripts to the symbols denote the relative degrees of the force or strength of the centroid as an effect in consciousness. They indicate the degree of the *entire centroid* and not that of the particular elements to which they are attached. A great number of variations in stress are possible, but the degrees have been limited to the first, second and third. Consciousness can immediately distinguish three degrees of auditory sensations: (1) high, medium, low; (2) loud, medium, weak; (3) long, medium, short.

Ellis,[1] has elaborated a scheme, whose main defect is its over-minuteness of analysis, in which five different elements (force, length, pitch, weight, and silence) of emphasis are distinguished. To each one of these elements he ascribes nine possible degrees of stress. The theory has been justly criticised, as well as misinterpreted.[2] The misinterpretation is due to a misunderstanding of the nature of the centroid. The centroid, as already affirmed, is not the resultant of the agglomeration of isolated fragments superimposed upon one another, but a complexity originally and inseparably multiplex. None of its elements exists in isolation. Nothing is added to a third degree centroid, except an intensification of one or all of its elements. Hence the scheme of Ellis provides for nine degress of stress, and not for forty-five,[3] as has been supposed.

A like over-minuteness of analysis is characteristic of the system of Sweet.[4] He distinguishes five degrees of quantity (very long, long, half long or medium, short, very short), three degrees of force (level, crescendo, diminuendo), four degrees of stress (very strong,

[1] Ellis *Accent and emphasis*, Trans. Eng. Philol. Soc., 1873–74 and June, 1876.

[2] Mayor, Chapters on English Metre, 57–74, London 1886.

[3] Mayor, Chapters on English Metre, 69, London 1886.

[4] Sweet, Primer of Phonetics, 43–66, Oxford 1890.

strong, half strong, weak), and three primary forms of intonation (level, rising, falling).

In Swedish[1] the following degrees are recognized: 4th, the principal accent of the *acute* species, *e. g.*, the first syllable of *anden* (from *and*); 3d, the principal accent of the *grave* species, *e. g.*, the first syllable of *anden* (from *ande*); 2d, the strong subsidiary accent, *e. g.*, the second syllable of *anden* (from *ande*); 1st, the weak subsidiary accent, *e. g.*, the first and third syllable of *universitet;* and 0, the weakest accent ("tonlöshet"), *e. g.*, the second and fourth syllables of *universitet.* No word has more than one principal accent (4th or 3d), nor more than one strong subsidiary (2d).

The groups in Tables I. and II. are arranged according to the order of frequency of occurrence. We shall consider them separately under the principles which they exemplify.

Principle of synthesis.—The several groups of Table I., which result from the different modes of combination of the elements, may be studied from two points of view: frequency of occurrence and degree of intensity.

The most frequent combination is , where loudness is slightly more prominent than duration and pitch. This constitutes 75.1% of all the groups. The next, the loudness-pitch fusion ($\overset{\wedge}{\circ}$), where loudness is distinctly predominant, represents 10.7%; the loudness-duration (•), 4.1%; the duration-loudness (•), 3.9%; the pitch-loudness (⁚), 2.8%; the duration-pitch (•), 1.8%; and the pitch-duration (•), 1.3%. The two extremes consist of the   and the • (pitch-duration). Perhaps it may be justifiable to regard the former as the normal, or most natural, and the latter as the abnormal, or most unnatural, mode of fusion. A fact scarcely less striking is that the loudness-pitch ( ) combination is almost equal in frequency to all the others, exclusive of the ○ centroid. In respect to frequency, therefore, the element of loudness stands preeminent. It is present as a distinctly apprehended element in all except two groups of combinations.

The following percentages, as to the degree of intensity arising from these fusions, obtain between the different degrees of each mode of synthesis:

	1°	2°	3°
	86.8%	12.1	1
⁚	12.2%	39.	48.7
•	00%	56.3	43.7
•	00%	93.3	6.6
∴	27.2%	54.5	18.2
•	14.3%	57	28.5
•	00%	80	20

[1] SUNDÉN, Svensk Språklära, 14, Stockholm 1895.

1. Of all the fusions the ○ (normal) centroid alone has the highest percentage in the first degree and the lowest in the third degree. All the others have the smallest percentage in the first degree, with the exception of $\overset{\circ}{\scriptstyle\Delta}$ (pitch-intensity).

Psychologically, this signifies that the normal or most prevalent mode of synthesis, though excelling in frequency, is vastly inferior in intensity. When an increase in intensity is effected, some one factor gains prominence.

2. The loudness-pitch ( ) synthesis alone has the highest percentage in the third degree. Hence this is the most effective mode of fusion in respect of intensity. The loudness-duration synthesis (●) is a close second.

3. All of the last five modes of synthesis of Table I. (loudness-duration, duration-loudness, pitch-loudness, duration-pitch, pitch-duration) have the highest percentage of centroids in the second degree. As second degree centroids, the duration-loudness and pitch-duration stand preëminent.

Thus far the elements have been considered in conjunction. No one element has been supreme. We now proceed to study the cases in which some one element predominates.

Principle of substitution.—The percentages of frequency are as follows (Table II.): loudness, 45.3 per cent.; duration, 42.3 per cent.; and pitch, 12.3 per cent. The proportion, though not as large as for the previous class, is seen to follow the same general order: the extremes are composed of the loudness and pitch centroids. In so far as these may be regarded as independent sources of intensity, loudness and duration are each employed nearly four times as frequently as pitch. Loudness is slightly more prevalent than duration.

The average length of the duration type was 0.28^s, which exceeds the average length of the normal centroid by 0.09^s. That is, it is 1.47 times longer than the average length of the ordinary emphatic syllable (falling under the principle of synthesis).

As to degrees of intensity, the following relations are sustained:

	1°	25.4%		1°	50.9%		1°	68.7%
	2°	50.8	●	2°	36.3		2°	31.2
	3°	23.7		3°	12.7		3°	00.0

Observations and conclusions.—A study of the two principles suggests the following conclusions:

1. The element of loudness ranks highest in the degree of intensity attainable through the employment of any one element as a substitutive

instrumentality ; duration is equal to about one-half the effectiveness of loudness, while pitch independently is inferior to duration. In some records these elements very rarely play an approximately independent rôle. This tends to heighten the effect, in harmony with the general principle of contrast, when they are fitly employed.

2. The chief function of duration and pitch as substitutive elements consists in producing a first degree centroid, or in constituting a doubtful syllable, as compared with an adjoining syllable, a centroid syllable.

Speculation may be indulged in as to the special functions, psychologically, of pitch and duration emphasis, considered as vehicles for expressing different states of thought and feeling. It may be suggested that the third degree duration emphasis seems specially adapted to express the emotions of awe and grandeur, as well as solemn and grave thoughts. Notice, for example, the frequent use that is made of the duration type of emphasis in the pulpit and on the rostrum (cf. record of C.O.S., below).

3. The chief function of loudness as a substitutive property, appears to consist in producing a second degree centroid : a like function seems to be subserved by the duration loudness (●̇), the pitch-duration (●̣), the duration-pitch (●̈) and pitch loudness (:) centroids. Loudness independently is equal to any one of these integrations in effectiveness of intensity.

4. The thought is suggested that the function of the coalescence of loudness and pitch (:), and probably of loudness and duration (●̣), lies in the production of a third degree, or maximum, centroid effect. It is the former mode of synthesis that displays a highest percentage in the third degree.

5. The function of the loudness-pitch-duration (○) mode of fusion, in which loudness is slightly the most important element, consists in serving as the substrate of the speech centroid ; and in constituting a syllable a centroid syllable when a constituent of a sequence of words.

Assuming ○ to be the threshold of intensity of the centroid, all syllables that fall below the threshold may be designated non-centroidal or weak syllables. They are lesser or fractional centroid syllables. There is a natural series of gradations below, as well as above, the threshold of the centroid. The threshold is always relative to any given speech record ; it is not a fixed quantity but is always more or less arbitrary.

6. A comparison of the two principles shows that the principle of synthesis, in respect to frequency of employment, sustains a relation to the principle of substitution, approximately as 3 : 1. The integrated centroids are employed about three times as frequently as the substitutive centroids.

7. In respect to the third degree of intensity, the proportion between the effectiveness of the mode of synthesis centroids and the mode of substitution centroids is as 1.54 : 1. For the second degree, the proportion is as 1.41 : 1 ; and for the first degree, as 1 : 2.47. In other words, the second and third degree integrated centroids are each about 50 per cent. more frequent than the second and third degree substitutive centroids. The first degree substitutive centroids, on the other hand, are about two and a half times more frequent than the integrated centroids. The centroids consisting of integrated elements are thus seen to be far superior to the centroids consisting of substitutive elements, as regards intensity.

8. To adequately account[1] for all the variety of force relations obtaining among centroids, two principles have to be invoked : the principle of substitution and the principle of synthesis. Both are important, but the last is the more effectively employed, both with reference to frequency and strength. The employment of both principles conduces to pleasing variety, richness and melody, and physical and mental economy in speech. A due observance of the two principles contributes no inconsiderable part to the enjoyment derived from listening to a good reading.

9. Although the essence of the centroid is always a threefold complexity, any one of the elements may become predominant, though not entirely supreme.

7. *Relation of the centroid to the parts of speech.*—A question which is immediately suggested by the foregoing considerations is the relation, both in respect of prevalence and degree of intensity, which the parts of speech sustain to the centroid. To determine this, 336 centroids were classified according to the kinds of words of which they were composed. The results are tabulated in Tables III. and IV.

TABLE III.

Distribution of the centroid among parts of speech.

Kind.	*n*	%
Nouns,	107	0.31
Verbs,	77	.22
Adjectives,	59	.17
Adverbs,	40	.11
Conjunctions,	21	.06
Prepositions,	16	.04
Personal pronouns,	11	.03
Proper nouns,	4	.01
Interjections,	1	.003

n, number of occurrences.

%, percentage of all occurrences.

[1] The element of quality, or timbre, has been left out of the account.

TABLE IV.

Relation of degrees of centroids to parts of speech.

Kind.	I°	II°	III°
Nouns,	0.54	0.37	0 09
Verbs,	.54	.35	.10
Adjectives,	.35	.44	.20
Adverbs,	.77	15	.07
Conjunctions,	.95	.00	.04
Prepositions,	1.00	.00	.00
Personal pronouns,	.54	.27	.18
Proper nouns,	.50	.00	.50
Interjections,	.00	.00	1.00

8. *Observations and conclusions.*—While the determinations are largely tentative, they point to a few facts which may be accepted as trustworthy.

1. The nouns and verbs rank highest, respectively, in grade of frequency. The adjectives come third, with a little over one-half the frequency of the nouns, and a trifle over three-fourths the frequency of the verbs. The interjections and the proper nouns rank the lowest.

2. Excluding the interjections, only one case of which was studied, all the different kinds of words have their highest percentage in the first degree, except the adjectives and the proper nouns. The latter are evenly distributed among the first and third degree centroids. The former have the largest percentage in the second degree, and also rank next to the highest of any part of speech in the third degree column. As between nouns and verbs, a parallel proportion obtains. Over half the number are first degree, a little over one-third are second degree, and only one-tenth are third degree centroids. The personal pronouns follow the same general course except that the percentage in the third degree is relatively about twice as high. In the adverbs, on the other hand, the gradation is very rapid from the I° to the II° ; the percentage of III° is about one-half of II° adverbs. The conjunctions and prepositions are almost wholly limetd to the first degree.

In the main, then, it seems that, as regards the intensive or stress aspect of words, the parts of speech rank in the following order: proper nouns, adjectives, personal pronouns, verbs, nouns, adverbs, conjunctions and prepositions. The proper nouns, however, may be considered doubtful, as only four cases were studied.

The subsumption according to degree of stress differs, it will be seen, from the subsumption according to the frequency of occurrence.

It is interesting to compare these results with the rules of accentuation propounded by GUEST,[1] according to which articles are rarely accented;

[1] GUEST, A History of English Rhythms, 85, London 1882.

qualifying words—for example, adjectives, adverbs, and possessive pronouns—receive a fainter accent than the words they qualify; personal and relative pronouns take a fainter accent than the verbs; and in words compounded of a substantive and adjective, the accent falls on the adjective. This theory is somewhat at variance with the above determinations.

SUNDÉN[1] holds that articles in Swedish are never accented. Pronouns, auxiliary verbs, prepositions and conjunctions are generally not accented.

It is quite apparent upon the surface, that, with reference to any part of speech, no rules can be laid down that admit of no exceptions. The same noun, adjective, verb, adverb, pronoun, etc., may at one time be a first, at another a second, and at yet another a third degree centroid, depending upon the relative importance which the word plays as a vehicle for the conveyance of the thought that is uppermost in the mind of the speaker on any given occasion.

9. *Recapitulation.*—The discussion of the preceding pages has been confined to a treatment of the centroid as an isolated element in speech—that is, a definition of the concept, the kinds, the elements, degrees and essence (as illustrated in the principles of integration and substitution), and the word-analysis of the centroid, have been given. The next step consists in studying the centroid as a constituent member in a sequence of centroids. This phase of our enquiry diverges in two directions. On the one hand, we have to consider the relations which centroidal and non-centroidal syllables sustain to one another as respects duration, whether that of equivalence, or of simple or complex proportion. This problem concerns the question of the "quantitative character of verse." On the other hand, we have to consider centroids as recurrences, and determine whether the time intervals between them are equal. This problem deals with the question of the succession of the centroids.

Both of the above problems are phenomena of time. Much confusion has obtained with reference to their relations, in writings on the "time-theory of verse." The signification of the term "quantity" has customarily been extended so as to become coextensive with all the time elements of rhythmical phenomena—succession, recurrence, duration, etc. "Quantity," however, is a phenomenon of duration; succession, a phenomenon of intermittent recurrence. The two are separate and distinct, and should not be made interchangeable terms in speech. Duration is independent of repetition; repetition involves duration. Duration, as synonymous with quantity in speech, is an uninterrupted span; succession, a series of long or short spans.

[1] SUNDÉN, Svensk Språklära, 15, Stockholm 1895.

The specific task of the first enquiry is to measure the time occupancy of elementary sounds, centroidal and non-centroidal, and the gaps occurring between them (not pauses). The function of the latter, starting with this given manifold, is to determine the exactness of the coördinations of the intervals which occur between the momenta of rhythmical wholes ; and to disclose the principles governing the rhythm of speech.

IV. The measurement of separate sounds and silences.

1. *Explanation.* Speech, structually considered, is a complex synthesis of a number of formal unities. The following may be distinguished : (1) the unity of the poem or discourse, as a whole, governed by the principle of the unifying actus of the theme ; (2) the unities of the sections and subdivisions, governed by the principle of the logical subordination of the parts to the whole ; (3) the unities—the unities of speech par-excellence—of the particular groups of words or syllables which are separated from other groups by pauses, governed by the principle of the pause—punctuation-mark or non-punctuation-mark pause—and the expiration interval ; (4) the unities of poetical verses, governed by the principle of the terminal pause ; and (5) the more ultimate unities, consisting of separate sounds or syllables, governed by the principle of the silences or glides which separate them, as dependent upon the action of the vocal organs.

The first two classes of unitary groups are easily recognizable upon the printed page ; in speech they are logically distinguishable. According to Sweet,[1] the only division actually made in language is that into "breath groups," corresponding to (3) above.

No account of the first and second kinds of unity has been taken in these experiments. The third (and the second in so far as it coincides with it) and the fourth receive separate treatment elsewhere. The fifth unity is the subject of this section.

The denial[2] of the existence of the unities of separate sounds and silences is founded upon a natural illusion. The unity of a grain of sand is for unaided discriminative visibility the grain as a whole ; for science it is the particles composing the grain. For the eye it is one thing ; for the microscope another. The only unitary sound groups in speech for unaided discriminative audibility may be the "breath groups." When the reproducing speed of the phonograph cylinder,

[1] Sweet, Primer of Phonetics, 42, Oxford 1890.

[2] Sweet, as before.

however, is sufficiently reduced these groups are split up into separate syllable groups.

The method by which separate sound unities are perceived is thus analogous to those used in perceiving ultimate sight unities, namely, to augment the quantity of the sensations by instrumentation.

The speed of the phonograph cylinder was reduced until a group of rapidly recurrent sounds, which had previously appeared as a homogeneity, was split up into discreet elements of sound separated by gaps. That is, psychologically these sounds and silences corresponded to a series of auditory sensations alternating with gaps or silences. It is the time of these several sensations, as well as the time of the intervals of silence between them, that was measured. Hence the terms separate sounds and silences are to be psychologically interpreted.

Physically, the sounds represent vibrations of given lengths, amplitudes and periods. The gaps or silences probably correspond in some cases to very weak vibrations (glides). Not infrequently, however, they are physically interpretable as absence of vibrations.

Physiologically, the sounds and silences are interpretable as changes in the condition of the vocal organs.

Owing to the degree of skill demanded by the difficulty of the work, the measuring was deferred to the later stages of the research. The words sometimes became so indistinct or transitory as to preclude measurement; but the instances were few, for the sounds, in reducing the rate of rotation of the cylinder, tended to become inarticulate before they became inaudible. A tone in order to be audible must consist of not less than approximately 16 vibrations per second. Theoretically, therefore, by slowing the speed sufficiently the lower pitches of the tones of the record would fall, as it were, below the threshold of audibility and become silent, before the higher pitches. The answer to this objection is that the degree of reduction was determined, not by the threshold of audibility, but by the threshold of articulability, which is higher than the former. Hence the sound which first became inarticulate determined the particular limit of the reduction.

The silences are indicated by dashes (—). The numeral below the dash indicates the duration of the silence in units of contacts; numerals above the syllables indicate the duration of the syllable to which they pertain. The unit of measurement in all the records is one contact, the value of which is $\frac{1}{32}$, $\frac{1}{37}$, $\frac{1}{40}$ or $\frac{1}{48}$ of a second, according as the recording speed was 80, 92, 100 or 120 revolutions per minute.

2. *Records.*

Subject, A. R. P. (Palmer, 1901.)

15 15 11 25
3 5 2 5 2 3 4 9
My bed—is—like—a—little—boat :—
4 2 4 3 5 15

15 18 14 19 48
5 5 8 6 3 9 6 11
Nurse—puts—me in—and I em—bark :—
8 5 4 3 33

22 ○ 23 ○ 19 ○ 29
2 7 6 6 6 8 4 8
She—girds—me—in—my—sail—ors—coat—
? 6 3 4 6 2 5 17

21 14
And—starts—me—in—the—dark.
? ? ? ? ?

(Robert L. Stevenson.)

Unit of measurement, $\frac{1}{40}$ of a second.

Explanation and characterization of record.—The subject, an admirer of poetry, was told to declaim so as to make the record express his highest conception of good poetical declamation. No insight was given into the nature of the problem to be investigated. The stanza was recited from memory.

To the ear the record possesses the qualifications of good verse ; the rhythm is markedly smooth, the modulation pleasing, and the centroids are emphasized with due discrimination.

Subject, W. C. Churchill, graduate student.

20 28 ○ 9 ○ 17 ○ 23
5 4 4 9 4 6 11 5
Oh—yet—we—trust—that—some—how—good—
2 4 7 7 6 3 5 9

19 18 21 ○ 40
6 4 3 6 4 13 3 14
Will—be—the—fi—nal—goal—of—ill,—
3 3 6 2 7 2 3 18

23 27 ○ 24 ○ 45
3 7 3 6 8 7 4 12
To—pangs—of—nature,—sins—of—will,—
2 7 5 13 8 4 24

23 ○ 46 ○ 22
4 7 4 12 3 8 3 10
De—fects—of—doubt,—and—taints—of—blood.
2 7 5 26 6 9 3

(Tennyson.)

Unit of measurement, 1/40 of a second.

Explanation and characterization of record. The subject was given a printed copy of the stanza, and, after a reading, was asked to read it into the phonograph as poetry. The record sounds much like the previous, save that the rhythm is not so regular. No peculiarity is manifest in the relation of the elements of the centroid. The delivery sounds natural; the rate was rather fast.

Subject, J. M. T. (Telleen, graduate student.)

```
 7     4    13    6     14    5    10  8
Take—the—joys—and—bear—the—sorrows.
    4    3    16    7     5    3

 3    7     11    6    6    4
Shut—those—eyes,—next—life—will—
    5     6     14    6    6    ?

 7     5     11    12     7    8
open,—stop—those—ears,—next—life—
 ?          7     4     33    7    6

 5    7      6   8     8 5
will—teach—hearing's—office.
   5    13          5
```

(Browning.)

Unit of measurement, 1/48 of a second.

Explanation and characterization of record. The same method of taking the record was used as in the W. C. record. These two sentences are selected from the complete stanza, the first because of its regularity of structure, the second because of its consecutive centroids.

The rhythm sounds less regular than in the previous record. The syllables are distinctly uttered; the centroids receive a good share of duration emphasis. The movement is rather slower than the subject's ordinary speed of talking.

Subject, E. H. T. (Tuttle, 1901, student of phonetics.)

Perfect scansion.

(*a*) *Without regulative concomitant.*

```
        24           31         20        33
 4    5   6     12     6    10   3     11
And—so—no—force,—how—ev—er—great,—
    ?   4    6      17     ?    3   3      11

         20           28         22     27
 3    9    2   13      4    14  3    8
Can strain—a—cord,—how—ev—er—fine,—
         5  4      11     ?    1   4     10
```

15 18 21 34
2 7 3 8 6 8 5 22
In to—a—ho—rizon—tal—line,—
2 4 4 6 2 5

22 21 24
3 7 4 6 3 7 4 11
That—shall—be—ab—so—lute—ly—straight.
? 7 3 8 3 5 6

(*b*) *With regulative concomitant.*

o 23 o 25 o 23 o 28
? 8 4 10 6 10 5 8
And so—no—force,—how—ev—er great,—
5 5 7 ? 7 12

23 o 24 o 24 o 23
4 11 4 11 4 8 3 13
Can strain—a cord,—how ev—er—fine,—
6 7 4 7 9

o 21 o 24 o 23 o 25
2 5 5 12 5 9 11 ?
In—to—a—ho—ri—zon—tal line,—
? 5 5 4 3 3 10

22 22 24
That shall—be ab—so—lute—ly—straight.
? ? ? ? ?

Unit of measurement, 1/48 of a second.

Explanation and characterization of records. The subject was informed that the distinctive purpose of the experiment was to determine how accurately the intervals between the emphatic syllables could be coordinated. He was told to scan the selection with the utmost regularity, first, without and, second, with the aid of the finger beating time upon the table. The delivery was rather slow. A second purpose was to determine the relation which long and short syllables would assume. Of the latter purpose, no intimations were given.

To the ear, the first specimen sounds like an excellent example of routine scansion; the second sounds still more artificial and mechanical.

The two records differ not so much in the relation of the component syllables and silences as in the regularity of the intervals; hence only the measurements of the syllables and silences of the former, which is free from mechanical regulation, are given in the table of results.

Subject, W. L. P. (Phelps, Prof. English Literature.)

3 7 10 8 9 8 4 12
But—one—mor—ning—the—min—i—ster,—
3 4 ? 6 ? 4 11

6 3 7 8 7 9
in—the—chapel,—gave—out—
5 ? 34 3 5

6 21 10 7 8 3 ?
the—hymn—num—ber—nine—ty—nine
3 35 ? 5 6 2

Unit of measurement, 1/48 of a second.

Explanation and characterization of record. The sentence is taken from the body of a prose record. The phrase rhythm is fairly-well preserved; the accents are normal, only one extra-long centroid occurring in the sentence. The rendition is distinct and emphatic.

(*Swedish poetry.*)

Subject, O. S. (Sandquist, age 13.)

16 22 19 25
5 10 4 8 3 11 5 5
Ack,—Herre—Jes—us—hör—min—röst,—
4 ? 4 5 3 5 9

17 18 17 32
5 9 4 8 3 7 ? 9
Gör—dig—ett—tem—pel—i—mitt—bröst,—
3 3 2 4 2 2 4 17

18 18 18 19
3 7 6 9 5 9 5 10
U—ti—mitt—hjär—ta—blif—och—bo,—
2 1 3 2 2 ? 3 9

18 22 15 42
Så har jag tröst och evig ro.

Unit of measurement, 1/32 of a second.

Explanation and characterization of the record. The subject was chosen from a number of speakers who appeared on a children's program, because of the monotone fashion of speaking. The verses were recited from memory. They are end-stopped, with three sectional punc-

tuation marks. The sounds and silences could not be accurately measured in the last line. There was a general tendency to let the voice fall in pitch and weaken toward the close.

To the ear the stanza has a rhythmic swing characteristic of the jog-trot method of declaiming of many children. The degree of emphasis is moderate, considered especially duration-wise.

(*Persian poetry.*)

Subject, K. H. K. (Kazanjian, graduate student, philosophy.)

```
 :3     30        ·2       23    •   15   ·  69
115  6    7     9     5   7   14   9   7   8
Ahi min—el—askh—i—ve—ha—la—ti—hi—
         5    5      6  ?    4    1   6   ?  25

       :   16          31        ·2 27 :       74
8     7   3  10   4   7   7 12   6   5   5
Eh—rak—i—kal—bi—be—hera—re—ti—hi—
   4      2  5    4   6    ?      7   4   2  32

      ·3       31         2   22 O2     21 ·:        71
11    8    6    8    10    5   7   7     11 6    9
Mo—naz—er—el—ayn—e —i —ie —ghayri —kum—
   4     4   3   4     2   4       5         5     22

      ·3     32       2     29       20
6     6    4    7    11   7    5   14    8 4 5
Euk—sum—i—bil—la—hi —ve—I—yatihi.
    7     3  4   6   3   2       ?
```

Unit of measurement, $\frac{1}{48}$ of a second.

Explanation and characterization of record. The primary object in studying the record was to determine whether or not the accent in Persian poetry is entirely lacking, as alleged, or, if present, what is its essence; and the character of the rhythm. The reciter, a native of Persia, was told to exercise care lest the rendition might be affected by our occidental mode of utterance. The verses were recited from memory.

The movement is fairly rhythmical; the melody strikes the ear as weird and unfamiliar. On careful listening the centroids were evident to the ear.

3. *Sounds.*—a. *Historical summary.*—Two chief views have been advanced as to the temporal values of emphatic and unemphatic syllables. According to the one, the relation sustained is that of equality. There is no necessary bond between emphasis and duration. As a matter of

contingency or as dependent upon the difficulty of the physiological process of articulation, either syllable may arrogate to itself supremacy of time value.

The second theory, which has been held in three forms, posits a relation of long and short between the emphatic and unemphatic syllables. According to the first form the relation is indefinite and variable. In this form the theory is held by GURNEY,[1] GOOLD, BROWN, LOTZE,[1] BOLTON[2] and a host of others. For the most part, the relation, while fairly constant, is held to be inexact. LANIER[3] may be taken as a representative of the second form, which maintains that the syllables of speech sustain a proportion of aliquot parts to one another. The proportion is always a simple one, such as 1, 2, 3, 4, 5, etc., and never fractional. According to the third form,[4] the ratio sustained is precisely as 1:2. The theory is generally applied to classic verse, which theoretically admits but two time values, long and short, as obtaining among its sounds.[5]

b. *The measurement of emphatic and unemphatic syllables.*—The relative amount of time-occupancy of weak and strong syllables is shown in Tables V. and VI.

TABLE V.

Duration of emphatic syllables.

Subject.	*d*	*a*	*p*	$\frac{p}{a}$	*n*
A. R. P.	2.17	0.16	0.03	0.18	13
W. C.	3.50	0.20	0.05	0.25	17
J. M. T.	3.08	0.17	0.04	0.23	18
E. H. T.	3.27	0.20	0.05	0.25	16
W. L. P.	2.20	0.20	0.05	0.25	11
O. S.	3.06	0.25	0.03	0.12	12
K. H. K.	2.95	0.18	0.03	0.16	16
Av.	2.89	0.19	0.04	0.20	14.7

Unit of measurement, 1^s. *d*, total duration; *a*, average duration; *p*, immediate probable error; $\frac{p}{a}$, relative immediate probable error; *n*, number.

[1] GURNEY, The Power of Sound, 443, London 1880.

[2] BOLTON, *Rhythm*, Am. Jour. Psych., 1893 VI 34.

[3] LANIER, The Science of English Verse, 73, New York 1880.

[4] POE, The Rationale of Verse, Works, VI 60, Chicago 1895. Elsewhere he maintains a contradictory position, 52.

[5] LANIER, The Science of English Verse, 113 New York, 1880.

ELLIS, The Quantitative Pronunciation of Latin, 8–20, London 1874.

TABLE VI.

Duration of unemphatic syllables.

Subject.	d	a	p	$\frac{p}{a}$	n
A. R. P.	1.15	0.10	0.03	0.30	11
W. C.	1.52	0.10	0.03	0.30	15
J. M. T.	0.93	0.11	0.02	0.18	8
E. H. T.	1.27	0.08	0.02	0.25	16
W. L. P.	1.31	0.13	0.04	0.30	10
O. S.	1.56	0.12	0.02	0.16	12
K. H. K.	3.85	0.13	0.02	0.15	28
Av.	1.65	0.11	0.025	0.23	15.7

Unit of measurement, 1^s; d, total duration; a, average duration; p, immediate probable error; $\frac{p}{a}$, relative immediate probable error; n, number.

(*a*) *Observations and conclusions.*—The tables of measurements point towards the following conclusions.

1. The absolute duration of any syllable is variable. The records reveal that the length of a given long or short syllable is never absolutely fixed: the precise length is different for every time it is spoken.

2. The average emphatic syllable is invariably longer than the average unemphatic syllable. The average of the former for all the records occupies 0.08 sec. more time than the average of the latter. The ratio is as 1.7 : 1; hence the average long syllable is almost one and three-fourths times as long as the average short syllable.

3. The ratio sustained between the average unemphatic and emphatic syllable varies with different subjects. The following are the ratios for each of the records, given in the order of the tables: 1 : 1.6; 1 : 2; 1 : 1.5; 1 : 2.5; 1 : 1.5; 1 : 2.08 and 1 : 1.38. With the exception of one record, all the average emphatic syllables are at least half again as long as the average unemphatic. The exception is the Persian; probably the element of duration is a property less prominent in the centroids of Persian poetry.

The highest ratio is furnished by the routine scansion record. Here the centroid syllable is two and a half times longer than the non-centroidal syllable. Scanned poetry thus appears to furnish the instance where the difference between the length of emphatic and unemphatic syllables is at its greatest. This suggests a conclusion with reference to the method in which classical Roman and Greek poetry was scanned.

The ratio in the prose record (W. L. P.) is slightly less than the average. Whether this is a valid distinction, as affecting prose and poetry, cannot, however, be maintained on the basis of this one record.

4. The ratios existing between individual emphatic or unemphatic syllables of the same or of different records, are neither invariable, nor are they ratios of simple proportion.[1] The possible relations, as may be seen by a glance at the records, and by an examination of the p column (the column of irregularity) are practically unlimited. Not infrequently the ratio is reversed, so that sporadic unemphatic syllables may be longer than the average of the emphatic syllables.

These facts incontestably refute the theory of "simple proportion." As an illustration, notice the following time relations in the consecutive syllables of the first line of the specimen of routine scansion, which, rhythmically considered, furnishes a type of poetry at its best: 0.083, 0.104, 0.125, 0.250, 0.250, 0.208, 0.062 and 0.229.

5. The long and short syllables are about equal in regularity. As respects the comparative amount of absolute irregularity, the average long syllable is one and six-tenths times more irregular than the short. The average amount of irregularity for each long syllable is $\frac{1}{400}^{s}$; for each short syllable, $\frac{1}{250}^{s}$. In proportion to the length of the span, however, the short syllable exceeds the long in the amount of irregularity by a trifle, namely, $\frac{1}{300}^{s}$.

6. The ratio between the number of emphatic and unemphatic syllables (exclusive of the Persian record) is as 1.2 : 1, which is smaller than the ratio between the total duration of the same. In none of the records are there less emphatic than unemphatic syllables, except in the Persian, where the numerical ratio is 1 : 1.75, and the duration ratio (the relative time-occupancy of all the emphatic and unemphatic syllables) as 1.33 : 1. The unemphatic syllables occur, as might be expected from the non-accentual theory of Persian poetry, one and three-fourths times as often as the emphatic. In English, however, the converse often occurs, as may be seen in the J. M. T. record. On the whole, the ratios between the duration and the number of long and short syllables do not coincide. The ratio between the latter may be equal; the ratio between the former is never that of equality for the total average duration of any record.

(*b*) *Bearing on the quantitative theory.*—The most general deduction to be made from this series of measurements is the disparity in the time-span of so-called short and long syllables. It has been shown that the temporal distinction is true to fact.

All speech is quantitative; and the distinction, popularly and confidently posited, between quantitative and non-quantitative verse, is grounded upon fallacious assumptions. The question of the quantitative character of poetry or prose, is closed. The only crux of contention that

[1] Cf. LANIER, The Science of English Verse, 73, New York 1880.

remains affects the character of the laws governing the temporal relation of the components of language. The question contains several phases.

The first determination has settled the fact that speech as a phenomenal occurrence is conditioned on the time-intuition of sensibility.

The second question has also been settled, namely that the strong syllable is temporally longer than the weak.

The third question is partly closed. It has been shown that for English, Swedish and Persian the ratio between the individual long and weak syllables is never constant. The general character of the ratio was, for artistically free declamation of English poetry, in the proportion of 1 : 1.7 ; 1 : 1.38 for Persian poetry ; 1 : 2.08 for Swedish poetry ; 1 : 1.5 for English prose ; and 1 : 2.5 for mechanical scansion.

It has generally been supposed that the ratio in classical verse is a fixed one, in the proportion of 1 to 2. Supposing the theory true, what justification is there for setting classical verse apart as *sui generis ?*

Evidently the distinction between so-called quantitative and non-quantitative poetry cannot be validated on the mere ground of proportionality, for our records have furnished examples of English and Swedish poetry in which the proportion is, respectively, as 1 : 2, and as 1 : 2.08 ; hence, if an examination of modern verse furnishes no support for the theory, on the basis of the *amount* of proportion (since it duplicates what the theory assumes as unique), the remaining alternative is to postulate a strict equality between each and every strong syllable and each and every weak syllable ; that is to say, to suppose that the Roman and Greek ear was capable of exactly coördinating every individual emphatic and unemphatic syllable so that, relative to one another, they should sustain the proportion of 1 : 2, and, regarded apart by themselves, a proportion invariably as 2 : 2 in the former, and 1 : 1 in the latter case. That such a proportion cannot be effected by the modern ear is indisputable. Even in the case of the strictest metronome scansion, the average inequality is 0.02^s for every weak, and 0.05^s for every strong syllable. When the modern ear is thus incapable of preserving the strict temporal equality of syllables, even in mechanical scansion, the quantitative theory as limited to classical poetry may, in regard both to *extent* and *invariability* of proportion, be considered exploded.

A third supposition is still possible, namely, that recited Greek and Latin verse, not improbably, was unique in the musical character of the utterance.[1] According to SYMONDS,[2] the poetry probably was sung or

[1] ELLIS, The Quantitative Pronunciation of Latin, 20, London 1874.

[2] SYMONDS, Sketches and Studies in Southern Europe, II 326, New York 1880.

chanted ; hence the tempo would have been an adagio, supposing ours to be allegro. This would naturally intensify the appreciation of the disparity in the durations of the two syllables, since more time would be afforded to the ear to listen to each syllable, and the quantity of sensation would be augmented. Hence the illusion would arise that the difference between classic and modern scansion of poetry lay in the relation of the weak and strong syllables, in respect of exact regularity and proportion, instead of in the relatively greater amount of time occupied by the syllables in classical scansion as compared with the non-classical, or reading, scansion of modern poetry.

The psychological explanation here advanced of the distinction between quantitative and non-quantitative poetry, as arising from a natural auditory illusion, seems quite compatible with the general psychological theory of illusions occurring in normal life.

(*c*) *Theoretical.*—How can we best account for the differences of duration existing between syllables?

The most prevalent theory may be called the physiological theory, which resolves the question into facility or difficulty of enunciation—into "lingual evolution." Long syllables are those that are encumbered by consonants,[1] or those that contain long vowels[2]; and short syllables are those unencumbered by consonants, or those containing short vowels. Hence the conclusion that there is no quantity[3] in English.

This theory undoubtedly explains a large percentage of cases. Some syllables, by reason of the vowels and consonants composing them, require a labored adjustment of the vocal apparatus, and hence a retarded utterance.

On the other hand, as was proved by the experiments of Král and Mareš,[4] the addition of consonants does not proportionately or regularly lengthen the syllable ; it often shortens it. Moreover, the theory applies equally well to both weak and strong syllables, and is hopelessly inadequate to explain the large array of instances where syllables are normally or regularly long, although easy of articulation. It fails to render a satisfactory account of the fact that the average emphatic syllable is invariably longer than the unemphatic. Only the psychological theory can satisfactorily account for this circumstance.

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[1] Poe, The Rationale of Verse. Works, VI 59, Chicago 1895.

[2] Guest, A History of English Rhythms, 103. London 1882.

[3] Guest, as before, v.

[4] Král a Mareš, *Trvání hlásek a slabik dle objektivní míry*, Listy Filologické, 1893 IV 17

According to GURNEY[1] the long syllable occupies more time than the short, because the amount of effort required for its pronunciation is greater than that required for the short syllable. Another theory, partly physiological, partly psychological, attributes the prolongation to the fact that when a special intensification is made it is *accompanied* by a retardation of the action of the voice apparatus. It requires a longer period to overcome the contraction; hence the sound is prolonged. Against this supposition it may be urged that the time required to *effect* the contraction is proportionately shorter; hence a neutrality of effect.

The most fruitful theory, we believe, is the centroid theory advanced above. The centroid is a mental center of force, of which duration is an inseparable element. It is constituted a centroid by reason of the prominence, proportionate or unproportionate, which is given by the speaker to its properties. It is as natural to intensify the duration factor, as the pitch and loudness factors.

4. *Silences*.—a. *Explanation*. By silences is here meant those gaps which in listening to a slowly revolving phonograph cylinder, are perceived to exist between the successive syllables of phrase groups. The gaps between phrase groups, or expiration intervals, may be called, in contradistinction to the former, vacant intervals or pauses. It is only the former with which we are now concerned.

The popular idea, says SWEET,[2] that we make a gap between every syllable is erroneous; the only phonetic divisions justifiable in speech are "those into sounds and syllables and intervening glides." It is patent, however, that the speech organs must consume a fraction of time in the transition from one sound to another, which will be long or short according to the inherent difficulty of the transition, and the amount of energy expended. A series of such transitions denotes, physiologically, a number of points at which the energy utilized by the organs of speech (diaphragm, lungs, larynx) may be considered to be at its lowest ebb; and, physically, points at which the vibrations of the air are diminished in amplitude and frequency. When the diminution reaches a certain point the transitions become, psychologically, gaps or silences. The perception of these, however, is a distinct problem. According as the mind is intensely absorbed in the thought element of the utterance, the gaps fail to rise above the threshold of consciousness, and hence it is natural to suppose that an expiration group of syllables is psychologically a continuum. The illusion is exposed when attention is directed to the purely structural aspect of

[1] GURNEY, The Power of Sound, 429, London 1880.

[2] SWEET, Primer of Phonetics, 42, Oxford 1890.

speech. The rapidity of ordinary utterance is so great as to render the elements of language too subtle for the processes of work-a-day analysis, and recourse must be taken to some means of aiding the power of mental analysis, namely, to magnifying the phenomena; this can be done by listening to a phonograph record, the rate of the rotation of the cylinder of which has been sufficiently reduced.

In a few instances it was impossible, in measuring, to detect any gaps between the syllables: they seemed to coalesce. In yet other instances, the gaps could be perceived, but were so indistinct as to make it hazardous to measure them. These instances are indicated in the records by a ?

In the table which follows a comparison is made between ante-, inter- and post-centroid silences. Only the average duration is given.

TABLE VII.

Duration of silences.

Subject.	—	·¦·	+
A. R. P.	0.103	0.200	0.103
W. C.	0.108	0.100	0.136
J. M. T.	0.095	0.139	0.104
E. H. T.	0.080		0.104
W. L. P.	0.079	0.072	0.111
O. S.	0.099		0.084
K. H. K.	0.086		0.078
Av.	0.092	0.127	0.102

Unit of measurement, 1^s.

—, silence preceding centroid.

·¦·, silence between centroids.

+, silence following centroid.

b. *Deductions.*—Owing to the limited number of silences upon which the determinations are based, the following are offered as tentative conclusions. Later researches may verify or modify them.

1. The silences that occur between centroids rank highest in the order of length.

It has been denied by GUEST[1] and affirmed by SWEET and MAYOR,[2] that two centroids can follow in an immediate sequence. The silence occurring between the contiguous centroids has been constituted the indispensable condition of such a series by some writers. GUEST, *e. g.*, affirms (p. 86) that the condition of the immediate sequence of two accented monosyllables, is the intervention of a "pause." When the words are in groups, the law affects only the group.

[1] GUEST, A History of English Rhythms, 551, London 1882.

[2] MAYOR, Chapters on English Metre, 29 ff., London 1886.

The inter-centroid silence has been conceived in the nature of a pause. This is untenable. These silences are essentially the same as those which precede or follow centroids. The difference in duration between the averages is almost negligible—0.025^s.

2. The post-centroid occupies more time than the ante-centroid silence. The converse has been affirmed to be true by writers in phonetics.

3. The average duration of the silences of speech is about 0.107^s.

5. *Comparison of the duration of sounds and silences.* (*a*) *Observations.*—The aggregate duration of all the sounds is equal to 31.82^s; of all the silences, 13.35^s. The ratio of quantity of silence to quantity of sound is thus in the proportion of 1:2.38.

This fact may contain material of importance of a psychological and physiological nature. It has been affirmed that the heart, for example, is alternately working and resting, resting between, working during, the pulsations. Similarly the alternation of sounds and silences suggest a constant alternation of work and rest on the part of the larynx.

There is this difference between the two cases. The period of work in cardiac action is about equal to, or slightly shorter than, the period of rest. In the action of the vocal cords, the period of work is nearly two and a half times longer than the period of rest. Obviously, this explains why the former mechanism is able to continue at work incessantly, while the latter requires long periods of rest for recuperating its energy. These periods are supplied partly by pauses, and partly by limiting the activity to a certain length of time, varying with each individual.

The above ratio is exclusive of pauses. In the later comparison of the various intervals of speech, the duration of the expiration intervals will be compared with the duration of the pause intervals.

None of the general averages of the silences is equal to the average duration of the centroid syllables. The average inter-centroid silence, however, is a trifle longer than the average short syllable.

(*b*) *Conclusions*—1. The quantity of the sounds of expiration intervals of speech is nearly 2½ times greater than the quantity of the silences.

2. The average emphatic and unemphatic syllables of speech are both uniformly longer than the average silences.

V. The measurement of intervals in speech.

A. Explanatory.

No little confusion has obtained in the meaning ascribed to the term meter. Some writers employ the term to signify exactly regulated rhythm;[1]

[1] Abbott and Seeley, English Lessons for English People, 146, Boston 1880.

others, the "foot,"[1] "measure,"[2] or "bar"; and others,[3] the scheme whereby a fixed number of stresses at fixed points is marked out for the verse, whence we derive the terms dimeter, trimeter, tetrameter, pentameter and hexameter. According to POE,[4] meter has to do with the number of feet, rhythm with the character of feet (the arrangement of syllables). Finally, meter has been made synonymous with the line[5] or verse group, whence our "long" and "short" meter. This is the only kind of meter, or rhythm, recognized by Japanese and Persian prosody, the poetry of both of these languages having, theoretically, no accent.

GUEST[6] provides, perhaps, the most comprehensive definition of the term. He makes it inclusive of three factors, according to the following scheme:

Meter $\begin{cases} \text{1. Elements—syllables, verses, staves.} \\ \text{2. Accidents—quantity, accent, modification of sound.} \\ \text{3. Law of succession.} \end{cases}$

Into a discussion of the propriety of these several views it is not our present purpose to enter. In so far as meter is synonymous with the verse and foot intervals, the two orders of meter will receive due consideration in the appropriate place.

The word verse is subject to a similar ambiguity. In prosody it is employed in three different senses: (1) A number of feet, or succession of words, constituting a metrical line. (2) A group of lines thus composed. (3) Metrical composition, as distinguished from prose.

The words foot, bar and measure are the popular words used to signify the intervals bounded by accented syllables.

The foot, in poetry, is generally defined as a succession of long and short or accented and unaccented syllables; the measure, as meter, or a rhythmical period; and the bar is a variant of the two. The first is exclusively applied to poetry; the second and third are also applied to music. In poetry they are generally employed as synonymous terms.

To designate the intervals of vacancy in speech the terms pause, rest, stop and silence are indifferently employed. The term rest finds favor in musical terminology; the word stop is synonymous sometimes with the punctuation mark and sometimes with the making of a change in pitch.

[1] MAYOR, Chapters on English Metre, 5 ff., London 1886.

[2] ARNOLD, A Manual of English Literature, appendix. Boston 1891

[3] GURNEY, The Power of Sound, 425, Lond. 1880; CORSON, ARNOLD, as before.

[4] POE, The Rationale of Verse, Works, VI 56, Chicago 1895.

[5] LANIER, The Science of English Verse, 234, New York 1880.

[6] GUEST, A History of English Rhythms, London 1882.

The sound sequence enclosed within two pauses may perhaps be termed a phrase.

What is the common characteristic of these various groups of facts—the verse, foot, pause and phrase sequences? Obviously, they all agree as partaking of the same temporal and spatial characteristics. They are phenomena of duration. The term interval may be used as a general word to denote any of these sequences. A verse interval is thus a sequence consisting of a line of poetry.

An interval extends between the opening and closing point of a given sequence. Spatially, it is the distance between the first printed object and the last printed object of the sequence. To illustrate: a verse interval is the distance upon the printed page from the first syllable to, and inclusive of, the last syllable of the line. Temporally, it is the time which is consumed in speech in passing from the first to the last sound of the sequence.

In the collection of records to follow, the explanation of the centroid symbols is the same as on p. 15.

The words enclosed in ( ) are inaudible on the phonograph record; those in [ ] were supplied by the subject.

The duration of the interval between any two centroids is indicated by the numeral which is placed midway between them. The measurements of the line and expiration groups are given only in the tables. Pauses marked ? are too indefinite for accurate measurement.

In the tables, "The immediate probable error" p is determined according to the formula:

$$p = \tfrac{2}{3}\sqrt{\frac{v_1^2 + v_2^2 + \cdots + v_n^2}{n - 1}},$$

where v_1, v_2, $\cdots$, v_n represent the differences that exist between each measurement and the average of all the measurements. The immediate probable error furnishes an expression for the amount of irregularity or inequality existing between the components of a series of measurements and the average of the series.

The "relative immediate probable error" $\frac{p}{a}$ furnishes an expression of the immediate probable error as a fraction of the average. It will be larger or smaller than the former according as the average is smaller or larger than the unit of measurement. The degree of regularity is expressed by this quantity.

Both the p and $\frac{p}{a}$ are measures of irregularity on the part of the subject. They are mental quantities. The inaccuracy of the average may be indi-

cated by dividing p by $\sqrt{n}$. This gives the "final probable error." It gives the limits within which the average of a similar set of measurements may be expected to vary with a probability of one-half.

The duration of the average interval may be represented to the imagination as a line of a definite length, *e. g.* ——— $= 1^s$. Every component of the given series of intervals will be either exactly equal to this line in length, *i. e.* 1^s, or will deviate from it in one of two directions—(a) plus (*e. g.* 1.50^s) or (b) minus (*e. g.* 0.50^s).

The e column gives for each record the lengths of the intervals which deviate the farthest from the average. Thus we get two kinds of extremes, — and +. The former represents the shortest component of the series, the latter the longest.

The c column gives the character of the difference of the greater of the two extremes from the average. To illustrate graphically:

+ ———————————— $= 2.^s$
Av. ——————— $= 1.^s$
— ——— $= 0.50^s$

The middle line is the length of the average; the line above is the longest, and the line below the shortest, single interval in the series. The differences between the average and the + line is 1^s; the difference between the average and the — line is 0.50^s. Hence the character of the greater extreme is +.

The cipher indicates that the extremes are equal.

The following records are explained and characterized upon the basis of the impression made upon the ear. They are classified, according to the style of delivery, into four types of poetical scansion.

The rhythmically free declamation of poetry, or reading scansion, corresponds to the rhythmically free reading of prose. The one is the basal type of poetical speech; the other, of prose speech. The former admits of several distinct varieties, the latter of two or three less distinct varieties.

B. Records.

First specimen, J. W. R. (James Whitcomb Riley, poet, lecturer.)

41 •2 29 30
They ain't no style about 'em,—
9

31 30 2 45 3 67
And they're sort o'pale and faded ;—
45

31 2 42 25 28
11 10
Yit the doorway here, without 'em,—
10

29 3 87 2 22
11
Would be lonesomer,—and shaded
41

19 32 2 30 2 22
7
With a good'eal blacker shadder

23 28 2 28 50
8 7
Than the mornin'-glories makes,—
41

31 2 45 37 2 40
10 10 12
And the sunshine would look sadder

39 2 38 34
8 9
For their good old-fashion sakes.—
67

2 43 37 2 28
I like 'em 'cause they kind o'

23 3 26 20 75
Sort o' make a feller like 'em ;—
41

3 20 2 16 20 22 34
11 8 11
And I tell you, when I find a

2 42 30 2 39 70
Bunch out whur the sun kin strike 'em,—
41

3 37 40 2 23
10
It allus sets me thinkin'

19 44 2 44 43
6.16 20 18
O' the ones 'at used to grow,

2 21 2 23 31 22
And peak in thro' the chinkin'

23 2 25 2 25
O' the cabin,—don't you know.—
? 65

2 36 26 2 64
And then I think o' mother.—
40

18 20 28 53
And how she used to love 'em,—
33

24 3 42 53
When there wuzn't any other,—
40

37 ○2 27 ○ 24 △ 78
'Less she found 'em up above (em) !—
53

40 ●3 43 ○ 35 ○2 58
And her eyes, afore she shut 'em,—
44

30 33 ●2 46 22
Whispered with a smile, (and) said—
8

34 26 2 55 2 19
We must pick a bunch—and put 'em
24

27 ○2 74 ○ 32
In her hand—when she was dead.—
37 69

20 24 ●2 11 56
But, as I was a saying,—
12

44 2 32 34
They ain't—no style about 'em—
8 19

● 29 ●2 55● 30 2 73
Very gaudy or displayin',—
53

34 ·₂ 20 27 60
Yet I wouldn't be without 'em,—
40

44 ·₂ 33 44 ·₂ 54
'Cause I'm happier in these posies,—
28

₂ 22 52 48
(And the) hollyhawks—and sich,—
15 39

25 21 38 •₃ 36
Than the hummin' bird,—'at noses
15

26 •₂ 49
In the roses of the rich.

(Old Fashioned Roses, Hoosier Dialect, J. W. Riley.)

Unit of measurement, $\frac{1}{48}$ of a second.

The poem was recited from memory. No suggestions were offered.

The style abounds in the pathetic and the grave. The movement is rather slow, with occasional diminuendos and crescendos and retarded and quickened pace. The melody and swing are peculiarly Rileyan. The rhythm is not continuous. The total effect upon the ear is pleasing.

The verses are largely end-stopped. Numerous sectional punctuation marks occur. Five stanzas were measured in order to obtain ample material for a study of the regularity of the verse intervals.

Second specimen, E. W. S. (Scripture.)

17 21 ·₂ 31
The cities are full of pride,—
20

·₂ 24 20 35
Challenging each to each—
29

⊙₂ 28 ○ 23 ○ 27
This from her mountain side,—
16

⊙₂ 20 ⊙₂ 19 ○
That from her burthened beach.

2 21 29 ·2 34
They count their ships full tale—
15

24 25 ·2 36
Their corn and oil and wine,—
25

25 ○ 20 ○ 40
Derrick and loom and bale,—
27

26 27 42
And ramparts gun-flecked line,—
31

21 23 32
City by city they hail :—
18

·2 19 23
Hast aught to match with mine?

13 ·2 21 27
And the men that breed from them—
12

15 18 3 41
They traffic up and down,—
25

2 24 18 30
But cling to their cities hem—
12

·3 23 17 2
As a child to the mother's gown.

25 ·2 21 41
When they talk with the strange bands,—
29

27 ○20 ○ 42
Dazed and newly alone ;—
24

25 ○ 22 ○ 43
When they walk in the stranger lands,—
30

∴₂ 22 ‐ 23 ‐ 33
By roaring streets unknown ;—
24

∴₂ 26 . 26 ‐ 39
Blessing her where she stands—
20

○₂ 19 ○ 19 ○
For strength above their own.

∴₂ 17 ‐ 24 28
On high to hold her fame—
9

●₃ 30 ○ 21 ○ 39
That stands all fame beyond,—
26

19 27 40
By oath to back the same,—
24

○₂ 26 ○ 19 ○ 54
Most faithful-foolish -fond ;—
42

△ 26 ●₂ 26 ○ 24
Making her mere breathed name

19 20
Their bond upon their bond.

∴₃ 19 29 ·₂ 26
So thank I God—my birth—
8 7

17 ∴₂ 27 32
Fell not in aisles aside—
24

12 15 · 16 ·₃ 31
Waste headlands of the earth,—
15

○₂ 21 ○ 29 ○ 30
Or warring tribes untried—
16

15 20 28
But that she lent me worth—
?

10 21
And gave me right to pride.

27 20 25
Surely in toil or fray—
15

19 25 35
Under an alien sky,—
24

18 19 24
Comfort it is to say:—
11

24 22
Of no mean city am I.

(Dedication to the City of Bombay, Kipling.)

Unit of measurement, $\frac{1}{48}$ of a second.

The first two stanzas were declaimed from memory; the others were read from the book. The verses are end-stopped. No sectional punctuation marks occur.

The record is typical of the subject, especially as regards emphasis, distinctness of articulation and rapidity of utterance. The point which most impresses the ear is the preservation of the rhythmic flow, both in respect of the recurrence of the centroids and the equality of the verse intervals. The last stanza has a peculiar jog-trot swing; it will be used in the study of sing-song rhythm. The rhythm of the other stanzas is, in places, so pronounced that the record could have been subsumed under the head of doggerel scansion. The thirty-five lines furnish ample material for studying the verse interval rhythm.

Third specimen, A. D. B. (Bissell, musical composer, graduate student.)

(A.)

23 32 38 36
Over the chimney—the night-wind sang—
7 15

25 ○ 22 ○13 ○13 ○2 77
And chanted a melody—no one knew;—
6 56

2 22 3 32 15 27 38
And the woman stopped,— as her babe she tossed,—
23 20

18 38 ●2 55 ● 60
15 12
And thought of the one she had long since—lost,—
18 38

2 65 2 34 3 29 81
And said,—as her tear-drops—back she forced,—
38 8 55

3 34 2 40
8 6
"I hate—the wind—in the chimney."
13 15

(What the Chimney Sang, Bret Harte.)

(B.)

25 2 30 3 30 2 27 40
Though the mills of God—grind—slowly,—
17 18 22

22 2 39 2 33 71
Yet they grind exceeding—small;—
? 56

28 ⊙3 41 ○ 29 ⊙ 42
Though with patience—He stands—waiting,—
15 ? 27

26 2 43 29
With exactness—grinds He all.
20 (Retribution, Longfellow.)

Unit of measurement, $\frac{1}{48}$ of a second.

The first stanza was recited from memory; the second, from the book. The subject was uninstructed. The verses are end-stopped, with sectional pauses.

The record presents a variety of smooth and rugged movements. The swing in some phrases is almost pendulum like. The centroids are well emphasized. The subject was intent, apparently, on forcibly and artistically expressing the thoughts, rather than guarding against interruptions in the flow. The movement is rather slow.

Fourth specimen, W. W. (Wallin.)

21 ●2 44 ●3 23 26 ● 43 ●2 19 43
The sky is changed!—and such a change!—Oh night,—
34 35 19

●3 50 ●3 42 17 31 43
And storm—and darkness,—ye are wondrous strong,—
21 11 28

●3 18 32 2 43 ● 20 ● 28 30
Yet lovely in your strength,—as is the light
33

● 14 ●2 24 45 20 32
Of a dark eye in woman!—Far along,—
35 13

23 35 22 25 ● 36
From peak to peak,—the rattling crags among—
18 25

27 18 ●2 55 24 ●2 14 ●2 36 30
Leaps the live thunder!—Not from one lone cloud,—
30 18

16 ●2 25 18 21 43
But every mountain now hath found a tongue,—
23

3 16 35 20 20 ● 43
And Jura answers,—through her misty shroud,—
14 32

2 28 21 41 19 33
Back to the joyous Alps,—who call—to her—aloud!
22 ? 22

2 17 14 80 ●3 41 2 54
And this is in the night:—Most glorious night!—
45 44

●2 12 2 11 11 22 58 2 14
Thou wert not sent for slumber!—let me be
37

18 ●3 28 15 ●3 36 ●2 17 53
A sharer in thy fierce—and far delight,—
10 40

2 30 2 28 15 ●2 51
A portion of the tempest—and of Thee!—
18 39

12 13 14 48 9 11 35
How the lit lake shines,—a phosphoric sea,—
30 28

11 14 14 13 25 10 52
And the big rain comes dancing to the earth!—
38

35 27 67 30 63
And now—again—'tis black,—and now,—the glee—
20 15 40 8 23

19 71 47 19 54
Of the loud hills—shakes with its mountain mirth,—
52 45

18 15 48 20 12 54
As if they did rejoice—o'er a young earth-quakes—birth.
33 23

(Childe Harold, Byron.)

Unit of measurement, $\frac{1}{48}$ of a second.

The stanzas were chosen as typical examples of Byronic vigor. The record strikes the ear as aiming more at painting the scene than moving in measured strides. The rhythm is vague. Several glides occur. The centroids are emotional, rather than logical. The elements of pitch, intensity and duration are each quite conspicuous. The verses afford an interesting study of the sectional and end-line pauses; several are run-on, with sectional pauses. Several of the larger punctuation marks occur within the line.

Fourth specimen, J. K. (Kawabe, graduate student, philosophy.)

16 19
Kimiga yo wa— 36
?

17 15 21
Chiyo ni yachiyo ni— 45
?

12 18 13
Sazare ishe no— 40
7

25 20
Iwao to narite 48

8 10 10
Koke no musu made. 41

(National poem of Japan.)

Fifth specimen, I. M. (Miyake, graduate student, psychology.)

9 0 9 17
Kimiga yo wa— 35
?

22 0 10 28
Chiyo ni yachiyo ni— 51
?

11 16 53
Sazare ishi no— 41
39

29 25
Iwao to nari te— 51
?

0 15 0 11 0 11
Koke no musu made. 52

Unit of measurement, $\frac{1}{48}$ of a second.

The numerals following each verse indicate lengths of the lines.

Both subjects are natives of Japan. The poem was recited (chanted) and declaimed, strictly following Japanese usage. The above are the records of declamation and not of song.

The line meter strikes the ear as being perhaps more prominent than the centroid or "foot" meter. Theoretically, Japanese poetry is non-accentual. The centroids, however, were easily located by the experimenter, and the result was verified by the judgment of J. K. Japanese admit emphasis, as distinguished from accent, but make the distribution wholly arbitrary. It will be seen, however, that both records correspond in regard to the location of the centroids, with the exception of one instance.

The only meter which Japanese prosody recognizes is the line meter, the above consisting of an alternation of the five-syllable with the seven-syllable type. The five verses constitute one sentence.

The following is a free English rendering of the poem: "May thy throne last forever, until the sands shall become rock and mosses shall grow thereon."

Sixth specimen, H. Ö. (Öhrnstedt, age 12).

$_{3}$ 20 $_{2}$ 30 $_{2}$ 21 38
I stilla glans—han träder fram :—
8 15

$_{3}$ 20 30 $_{2}$ 19 48
Af spridda får—och späda lam—
11 28

$_{2}$ 20 27 20 38
Han sig en hjord—församla vill,—
12 18

19 20 $_{3}$ 20 55
Den himmelriket—hörer till.—
? 33

$_{3}$ 22 20 $_{2}$ 20 37
Och himmel skall förgås och jord,—
19

20 19 $_{2}$ 20 42
Men ej hans helga dyra ord,—
22

$_{3}$ 22 21 21
Hans namn af alla tungorsljud—
20

39 ○ 21 ● 20 ○ 20 ○
Skall kallas Frälsare och Gud.

Unit of measurement, $\frac{1}{40}$ of a second.

The record was taken for the purpose of studying sing-song. The rhythm is very marked.

The end-stopped verses predominate. No intra-line punctuation marks occur.

Seventh specimen, O. S. (Sandquist, age 13).

○ 20 ○ 22 ○ 19 ○ 26
O. Jesu Krist—att nalkas dig—
? 11

○ 16 ○ 19 ○ 39
Och dig i tro tillbedja,—
18

21 21 20 26 26
Det endast kan på sorgens stig—rätt innerlig—
10

For first stanza and explanation see p. 28.

This record furnishes an excellent specimen of the doggerel scansion so common to children.

Eighth specimen, G. A. A. (Andreen, lecturer; professor of Scandinavian languages.)

2 16 16 2 27 14 31 19 3
Washington was a statesman of the highest order,—never a wily
18

19 ● 2 15 11 2 42 16 3 20 20 2 10 25
scheming pol i tician.—When a cabal was formed—to remove him from
32 ?

16 2 16 47 3 15 14 16 2 20
the command and elevate Gates,—in perfect equipoise of mind and
27

25 2 23 59 2 25 16 52
temper,—he moved not a finger.—Neither was he a brilliant orator.—
6 47 33

10 28 13 20 2 15 40 3 25
So that his excellence consisted not in show and glitter,—but in nobleness
24

48 18 13 34 20 59
of heart,—solidity of understanding,—and tenacity of purpose.—No
23 14 37

16 25 15 18 29 18 35
wonder then—that every American feels—as if it were true,—that "take
16 19 21

20 13 32 9 12 10 15 12 41
him for all in all,—we shall not look upon his like again,"—that none
18 23

25 11 15 34 26 25 42 31
do we more love to honor,—none so impresses our souls,—as he,—"the
23 23 15

18 32 20 38 24 23
first in war,—the first in peace,—the first in the hearts of his countrymen."
17 21

Unit of measurement, $\frac{1}{48}$ of a second.

The address was read from the manuscript. The style was rhetorical; the utterances rapid and forcible. The subject was asked to make the record a typical address to an audience.

Tenth specimen, W. L. P. (Phelps, lecturer; professor, Eng. Lit.)

12 18 62 20 17 29 34
The class of ninety-nine—did not hold a very high reputation—as
22 12

70 38 20 27 80 37 21
scholars,—either for—study or for morals.—And it was notorious that
23 13 70

21 17 17 26 21 93 11 34
they paid no attention to the Sunday services;—but one morning, the
56

41 14 54 11 24 55 22 20 84
minister,—in the chapel,—gave out the hymn—number ninety-nine!—
13 34 36 74

20 16 55 35 25 17 48 20
And with one accord,—the students of the class of ninety-nine—opened
32 34

40 19 77 16 17 34
the hymn book,—to find the place—and what was their amazement, on
13 52

21 3 20 2 65 20 2 16 70 2 25 3 45
reading the first—line,—number ninety-nine; — great—God! —what
? 49 53 10 16

2 43 2 40
worthless—worms are we!
15

Unit of measurement, $\frac{1}{48}$ of a second.

The words were spoken extemporaneously. The movement is, on the whole, slow; the articulation distinct; the style colloquial. The sentences were punctuated by the speaker.

Eleventh specimen, C. O. S. (Scoville, clergyman, graduate student, philosophy.)

2 17 2 11 13 13 2 20 2 23 22
Almighty God, unto whom all hearts are open,—
7

2 32 2 25 34 28 11 16 34
all desires—known,—and from whom no—secrets are hid;
9 15 8

44 25 2 17 13 31 20 33
—Cleanse the thoughts of our hearts—by the inspiration of
39 8

20 35 18 26 3 36
thy Holy—Spirit,—that we may perfectly love—thee,—and
14 27 ? 19

15 32 2 14 16 45 34
worthily magnify— thy holy Name;—through Christ—our Lord.
11 25 12

2 33 29 29 34
Lord,—let me know mine end, and the number of my
?

20 21 11 28 28 24 58 2
days:—that I may be certified how long I have to live.—Behold,
7 36

23 26 27 30 22 18 50
—thou hast made my days as it were—a span—long:—and my
7 12 ? 16

• 21 27 2 16 20 21 • 46 19 30 •
age is even as nothing in respect of thee;—and verily every man
27

25 • 28 19
living is al—together—vanity.
? 7

Unity of measurement, $\frac{1}{40}$ of a second.

Both prayers were read from the manual. The latter is offered at the burial of the dead. The movement is slow, the melody solemn, the intonation monotonous, the style that of devout consecration or solemn importunation, and the rhythm quite manifest in certain phrases. The duration centroids are prominent.

Test records.—The rhythmic feeling has been asserted to be the most general æsthetic endowment of man.[1] Most people undoubtedly distinguish spoken verse from prose largely on the basis of this feeling. The external embodiment of verse (aside from the nature of the thought), that is, its verse and stanza-like structure as it appears in print, may be presumed, however, from the law of association, to play no inconsiderable part in lending support to the distinction. How, now, would the judgment of the individual be affected after the props of association have been removed? How would naïve, unsophisticated thought regard the distinction of prose and poetry into measured and unmeasured language.

To get at the uncritical, spontaneous judgment of the individual, the following test was devised:

The subject was conveniently seated before the phonograph, and was given a short passage in print, the contents of which were unknown to him, to be immediately read into the phonograph. He was urged to make his reading natural and representative.

Two devices were employed. In the first the subject was given a selection of poetry printed in the form of prose; in the second, a passage of melodious prose printed as a stanza of poetry. In the latter case the subject was allowed to read the stanza through, before speaking into the tube, and was then told to read it according to his own discretion.

After the passage had been read the following questions were asked:

1. Did the passage, as you read it, sound familiar?

2. What did you spontaneously conclude regarding its form? If the question had never been raised, would you have taken it for granted that you read a piece of poetry or prose? What did you feel it to be?

[1] LANIER; GURNEY, as before, 128 ff.; BOLTON, as before.

3. Without engaging in reflective analysis, what do you instinctively feel to be at the basis of your judgment? Why poetry? Why prose?

4. Did the passage sound smooth or rugged as you were reading, or was it indifferent?

5. What did you feel these properties to be due to?

6. Did the passage sound melodious to you? If so, what does melody in speech mean to you? What are its elements?

Care was taken lest the subject should begin to unduly reflect upon the questions. Those judgments were rejected where the subject was familiar with the quotations. The questions were submitted to several persons from whom no records were taken.

The questions are in no wise to be considered as a statistical enquiry. They are only intended to reveal the state of consciousness, or the spontaneous judgments, of unsuspecting subjects.

After the answers had been given and the device exposed, the subjects of the first experiment were handed a copy in which the poetical quotation was printed in its original form,—that is, as poetry. The subjects were then asked to familiarize themselves with the contents, and read it as poetry. Thus it was possible to make a comparison of the two renderings by a study of the phonograph records.

A third device consisted in eliminating all the punctuation marks. The subject was not allowed to examine the passage before speaking it into the recording tube.

(*a*) *Records of verse.*

Twelfth specimen. J. M. T. (Telleen, graduate student, English.) (A, *without punctuation marks.*)

$\cdot_2$ 16 14 25 25 41 28 • 40
Nothing henceforth—man's existence—bows—[bows] to the mo—ni-
11 29 24 20

8 . 16 30 25 24 26 9 17 $\bullet_2$ 27
tion wait take the joys and bear the sorrows neither with extreme con-

25 · 15 • 35 17 20 $\cdot_2$ 40 20 $\cdot_2$ 13 $\cdot_2$ 30
cern—living here—means nescience simply—'tis [the] next life—that
12 24 25 10

· 15 18 · 9 • 14 • 21 $\bullet_2$ 13 39 $\cdot_2$ 27 · 12 • 14 • 27
helps to learn—shut those eyes—next life—will open—stop those ears—
7 8 29 12 13

$\cdot_2$ 12 33 19 20 37 • 14 • 13 26 14 11
next life—will teach—hearing's office—close those lips—next life
17 11 23 17

16 35 35
will give the power of speech!

(B, *read as prose.*)

·2 21 ● 15 14 21 · 25 ·2 28 ○ 7 ○ 26
Nothing!—Henceforth man's existence—bows to the mo nition—
8 5 8

·2 32 18 ·2 28 ●2 29 30 · 12 ○2 32 ○ 20
wait—! Take the joys and bear the sorrows—neither with extreme
23 13

39 ·2 10 ● 16 18 · 17 · 35 ● 12 ○ 11 ○ 23
concern!—Living here means nescience simply:—'tis next life that
26 25

21 ●2 41 · 11 10 ●2 32 · 12 · 20 · 41 ●2 36 ○ 9
helps to learn.—Shut those eyes,—next life—will open,—[those] stop
29 20 6 23

● 19 41 ●2 20 ● 31 15 ●2 24 ○ 40 ·2 8 ○ 7 ⊙2 37
those ears,—next life will teach hearing's office,—close those lips,—
27 27 25

· 16 · 25 23 · 33 ·
next life will give the power of speech!

(C, *read as poetry.*)

·2 23 52 ● 20 · 20 · 26 ·2 35 ○ 23
Nothing!—Hence—forth man's existence bows to the monition—
10 44 8

43
wait—!
35

· 17 ●2 42 24 ●2 37 · 12 · 20 ○ 25 ○ 40
Take the joys—and bear the sorrows—neither with extreme concern!—
20 28 34

· 18 ●2 26 15 ●2 19 · 43 · 13 · 13 ○ 24 ○ 20
Living here—means nescience simply:—'tis next life that helps to
? 30

○ 50
learn.—
42

·3 9 ●2 13 ●2 50 ·2 13 · 19 ● 48 ·2 12 · 17 ●2 46 ○2 12 ·
Shut those eyes,—next life will open,—stop those ears,—next life
34 33 37

30 ● 22
will teach

△ 16 50 ∴2 8 2 13 ∴2 37 △2 12 28 27 36
Hearing's office,—close those lips,—next life will give the power—of—
37 28 ? ?

○
speech.

(Browning.)

Unit of measurement, $\frac{1}{48}$ of a second.

Answers to questions.—1. No. 2. Prose. 3. Form of print. 4. Rugged. 5. The thought; loose rhythm. 6. No.

Thirteenth specimen. C. O. (Olson, graduate student, English.)

(A, *read as prose.*)

○2 32 30 22 29 ∴2 29 9 21
Nothing!—Henceforth man's existence—bows to the mon ition—
18 6 11

○3 39 18 24 ●2 23 47 11 20 △ 21
wait!—Take the joys and bear the sorrows—neither with extreme con-
33 31

△ 46 14 16 40 26 41 12 △2 13 ∴ 25
cern!—Living here means—nescience simply:—'tis next life that
38 27 26

∴2 19 ● 39 ∴2 12 ∴ 14 ●3 29 12 ●2 19 39 ∴2 12 ∴2
helps to learn.—Shut those eyes,—next life will open,—stop those
31 18 25

11 ● 25 25 23 14 ∴2 19 43 ●2 16 ● 16 ∴3 25 11
ears,—next life will teach hearing's office,—close those lips,—next
15 26 15

● ∴2 24 12 ●2 25
life—will give the power of—speech.
6 9

(B, *read as poetry.*)

○2 46 16 ● 18 21 26 29 21 2 41
12
Nothing!—Henceforth man's existence—bows to the monition—wait!—
30 10 10 32

○ 12 ●2 32 ∴2 19 ● 51 2 9 17 22 49
10 11
Take the joys—and bear the sorrows—neither with extreme concern!—
15 32 35

△ 10 ●2 20 13 ∴2 23 34 11 ∴2 12 20 18
16
Living here means nescience simply:—'tis next life that helps to
22

○ 42
learn.—

30
8 9 ● ·2 23 12 ·2 20 · ·2 23 ·2 8 ●2 12 ●2 24 ·2 9 ⊙
Shut those eyes,—next life will open,—stop, those ears,—next life—
9 18 9 31

47 21
will teach—
11

·3 16 41 8 9 26 ·3 12 ●2 21 ○ 16 ⊙●2
Hearing's office.—close those lips,—next life will give the power
28 18

31 ○
of—speech!
16

Unit of measurement, $\frac{1}{48}$ of a second.

Answers to questions.—1. No. 2. Dramatic prose. 3. Did not "feel" it to be poetry. 4. Smooth. 5. The movement. 6. Yes. Smoothness and movement.

Fourteenth specimen. G. F. A. (Abel, graduate student, philosophy.) (*Without punctuation marks.*)

●2 12 ●2 17 ·2 30 ·3 29 · 37 ·3 19 ○ 17
Henceforth man's existence—bows to the monition—wait—take the
14 26 10

·2 25 ●2 19 ·2 39 10 · 24 ·3 24 ○2 10 ○2 14
joys and bear the sorrows—neither with extreme—concern—living
25 8 ?

18 ? 11 ·2 22 52 ●·2 14 ·3 31 ○ 17 ● 41 ○2
here means nescience—simply—'tis next life—that helps to learn—shut
7 33 16 31

12 ·2 10 ● 22 ·2 10 ·3 15 · 36 · 9 ● 13 ● 37 ○2 12 ○ 19 ○ 10
those eyes—next life will open—stop those ears—next life will teach
8 25 24

·3 15 39 ●2 12 ●2 15 · 33 12 18 · 17 ○ 29
hearing's office—close those lips—next life will give the power of—
26 26 8

○
speech!

Unit of measurement, $\frac{1}{40}$ of a second.

Answer to questions.—1. No. 2. Prose. 3. Arrangement of words; form; lack of rhyme. 4. First part, rough; second part, smooth. 5. Lack of meaning and punctuation marks. 6. Yes and no, as in 4. Rhythm, if anything.

Fifteenth specimen. W. C. (Churchill, graduate student, philosophy.)
(A, *read as prose.*)

18 23 9 15 23 16 15 17
Oh yet we trust—that somehow good will be the final goal of ill,
5

49 23 24 21 53 19 25 21
—to pangs of nature,—sins of will,—defects of doubt,—and taints of
39 12 45 19

56 21 30 21 57 11 14
blood;—that nothing walks—with aimless feet;—that no one life—
50 18 49 9

24 15 40 22 19 11 49
shall be destroyed—or cast as rubbish to the void,—when God hath
31 41

24 ○ 18 ○ 22 ○
made the pile complete.

(B, *read as poetry.*)

20 28 9
Oh yet we trust—that somehow—good—
13 ? 9

23 19 18 21 40
Will be the final goal of ill,—
37

○ 23 27 ○ 24 ○ 45
To pangs of nature,—sins of will,—
15 37

23 46 22 61
Defects of doubt,—and taints of blood;—
34 48

18 23 20 48
That nothing walks with aimless feet;—
43

10 13 27 19 36
That no one life—shall be destroyed,—
11 33

27 22 14 48
Or cast as rubbish—to the void,—
? 34

26 23 41
When God hath made the pile—complete.
16

(Tennyson.)

Unit of measurement, $\frac{1}{40}$ of a second.

Sixteenth specimen. B. S. G. (Gowen, college senior.)

(*Without punctuation marks.*)

25 22 28 ● 2 17 2 29 14 16 ● 41 ○
Oh yet we trust that somehow—good—will be the final goal—of ill—
7 12 25 5

22 24 39 60 30 18 2 28 ○ 23 ○
to pangs of nature—sins—of will—defects of doubt and taints of blood
26 35 19

32 15 2 42 ● 19 35 8 △ 2 13 ○ 23 ○
—that nothing walks—with aimless feet—that no one life—shall be
20 25 18 5

16 40 22 19 11 49 2 24 ○ 18
destroyed—or cast as rubbish to the void—when God hath made the
21 41

22
pile complete.

Unit of measurement, $\frac{1}{40}$ of a second.

Answers to questions.—1. No. 2. Prose. 3. "Felt" no rhythm. 4. Quite smooth. 5. Smooth succession of words; occasional rhythm. 6. In parts. Fitness of language to thought, and character of thought.

Seventeenth specimen. S. (Smith, laboratory mechanic.)

(A, *read as prose.*)

19 25 24 17 18 19 17 ○
Oh yet we trust that somehow—good will be the final goal of ill,—
5 42

50 24 33 20 33 21 32 ○ 19
to pangs of nature,—sins of will,—defects of doubt,—and taints of
18 25 23

37 18 30 23 50 ○ 11 △ 2 12 ○
blood;—that nothing walks with aimless feet;—that no one life
32 39

21 32 ● 53 25 29 66 ○ 16
shall be—destroyed—or cast as rubbish—to the void,—when God hath
19 30 12 53

20 23
made the pile complete.

(B, *read as poetry.*)

42 25 51 33
Oh yet—we trust that somehow—good
18 27

○ 20 ○ 21 ○ 34 ○ 58
Will be the final goal of ill,—
45

○ 26 ○ 45 ○ 20 ○ 34
To pangs of nature,—sins of will,—
29 27

○ 22 ○ 40 20 59
Defects of doubt,—and taints of blood;—
25 43

19 29 ○ 22 ● 47
That nothing walks—with aimless feet;—
? 29

○ 10○29 ○ 25 ○ 18 ○ 36
That no one life—shall be destroyed,—
7 22

○ 23 ○ 43○? 17 ○ 49
Or cast as rubbish—to the void,—
23 32

29 19 24
When God—hath made the pile complete.
7

Unit of measurement, $\frac{1}{40}$ of a second.

Answers to questions. 1. No. 2. Prose. 3. Felt little rhythm. 4. Not very smooth. 5. Form of expression. 6. Yes. Rhythm and vowel sounds.

Two other persons replied to the questions, but no records of their recitations were made. (B) 1. No. 2. Poetry. 3. Rhythm, rhyme and sentiment. 4. Smooth. 5. Metrical perfection, uniform length of words, avoidance of sibilants. 6. Yes. Meter, onomatopoetic words, o sounds, polished expression of sentiment. (C) 1. No. 2. Prose. 3. Form of print. 4. Smooth. 5. Rhythm and rhyme. 6. No.

The above two specimens of poetry are taken from the two foremost poets of the century just closed, the one standing for the highest metrical perfection, the other emphasizing the content rather than the formal investiture of thought. Each instance contains one case of run-on verses. The Browning selection, however, contains by far the greater number of intra-line punctuation marks.

To the ear the rhythm of the prose and poetry renderings appears to be about equally balanced. Appeal was made to several ears. On the

basis of the rhythm the attempt to distinguish the prose from the poetry in the readings did not prove very successful.

The movement in the records without the punctuation marks appears the most rugged. In these records the pauses strike the ear as if placed with diffidence and hesitancy.

(*b*) *Records of prose.*

Eighteenth specimen. S. I. (Isakson, stenographer.)

•₂ 19 26 13 ₂ 20 47
To the poor Christian that sits bound in the galley ;—
28

₂ 20 27 17 32 14 ○ 39
To despairful widows,—pensive prisoners,—and deposed kings ;—
13 12 29

21 ₂ 28 30 15 ○ 50
To them whose fortune runs back—and whose spirits mutiny—
7 21

19 ₂ 23 29 14 ₂ 17 ○
Unto such—death is a redeemer,—and the grave a place for rest.
9 10

(Essay on Death, Bacon.)

Unit of measurement, $\frac{1}{40}$ of a second.

Answers to questions.—1. No. 2. Poetry. 3. Form of print, easy movement, and beautiful sentiment. 4. Smooth. 5. Meter. 6. No.

Nineteenth specimen. O. E. S. (Staaf, graduate student, Latin.)

₂ 11 28 13 7 10 38
To the poor Christian that sits bound in the galley ;—
21

16 26 19 38 ○ 18 ○ 31
To despairful widows,—pensive prisoners,—and deposed kings ;—
12 14 20

₂ 29 21 14 17 ○ 43
To them—whose fortune runs back and whose spirits mutiny—
8 15

₂ 21 25 29 15 15
Unto such—death is a redeemer,—and the grave a place for rest.
14 10

Units of measurement, $\frac{1}{32}$ of a second.

Answers to questions.—1. No. 2. Poetry. 3. Meter, outward form. 4. Neither. 6. No.

Answers to questions were obtained in several cases without taking phonograph records. (D) 1. No. 2. Poetry. 3. Periodic recurrence of accents. 4. Smooth. 5. Rhythm. 6. No. (E) 1. No. 2. Doubtful. First two lines poetry; last two lines upset judgment. 3. Rhythm of first two lines (poetry); prosaic thought (prose). 4. Feeling of smoothness gradually vanishing. 5. Correspondence of main stress-points in first two lines (3 each). 6. Yes. Rhythm. (F) 1. No. 2. Poetry. 3. Verse-like structure, inversion, a certain rhythm. 6. Yes. Don't know. (G) 1. No. 2. Poetry. 3. Mechanical form and poetic sentiment. 4. Smooth. 5. ?. 6. Yes. (H), poet 1. No. 2. Rhythmical prose. 3. No meter. 4. Smooth prose. 5. Swinging movement. 6. Yes. Graceful expression of thought, alliteration and interior rhymes. (I) 1. No. 2. Poetry. 3. Meter and poetic language, especially the second line. 4. Not altogether smooth. 5. Smooth flow. 6. Yes. Pleasing sounds. (J) 1. No. 2. Both. Lines 2 and 4, poetry; lines 1 and 3, prose. 3. Rhythm and lack of rhythm. 4. Lines 2 and 4, smooth. 5. Rhythm. 6. Yes. ?

The second specimen of rhythmical prose arranged as a stanza of poetry is found on p. 26. The questions elicited the following replies: (K) 1. No. 2. Poetry. 3. Rhythm. 4. Smooth. 5. Regular recurrence of accents. 6. Vague feeling of melody. (L) 1. No. 2. Poetry. 3. Regularly recurring accents (primarily), rhyme, printed form. 4. Fairly smooth. 5. Rhythm. 6. No. (M) 1. No. 2. Poetry. 3. Rhythm and rhyme. 4. Smooth. 5. Uniform number of syllables to line. 6. Yes. Rhyme. (N) 1. No. 2. Prose printed as verse. 3. Subject-matter prosaic. 4. First part smoother. 5. Rhythm and rhyme. 6. No.

The judgments were derived from persons who, according to admission, possessed more or less poetic appreciation. Nearly all were admirers of music and the majority possessed some knowledge of its rudiments. The answers throw some light on the varied manifestation of the instinctive or native appreciation of rhythm, smoothness and melody in speech. The connotation of the term melody of speech is in most people's minds shifting and vague. This is suggested by the answers and by a cursory glance at the popular discussions of the subject. The word answers, in most people's minds, to no fixed conception. To most subjects it is interchangeable with rhythm (motion); to some, with vowel sounds, rhyme and alliteration (tone-color); to others, graceful and fit expression of thought; and yet to others the term has no signification.

The connotation of the term smoothness of speech is more fixed than that of melody. This quality was felt to be due to the quality, first, of

the movement, and second, of the sound. In some cases no definite notion corresponded to the term ; in others, no smoothness was felt. In some of the latter cases, as was noticed by the experimenter, the reading was rugged.

The success of the deception varied. Three subjects declared the verses of Tennyson and Browning to be prose. The reasons given were "lack of rhythm," "form of print," "indefinite." Only one subject pronounced the verses of Browning, and two the verses of Tennyson, poetry. All the judgments were based on the feeling of rhythm. Six subjects called the prose sentence of Bacon poetry. The reasons given were the outward form, sentiment, poetic language, inversion of words, or rhythm. One subject was doubtful, one pronounced the sentence half-and-half and one (a poet) pronounced it, correctly, rhythmical prose. The specimen of mathematical prose was declared to be poetry by three subjects on the basis of rhythm, rhyme, or the form of print ; and to be prose written as verse by one on the basis of the prosaic subject matter.

Some of the subjects, who felt no rhythm in the lines, pronounced the passages smooth, others feeling no smoothness pronounced the lines rhythmical.

The following corollaries, to be held tentatively, are deducible from a comparison of the judgments :

(*a*) Poetry is spontaneously and instinctively distinguished from prose most largely upon the basis of a rhythmic affection of sensibility. All other differences, such as inversion, sentiment, rhyme, etc., are subordinate.

(*b*) For unsuspecting and naïve thought, the arrangement of the words into verses and stanzas serves as an important prop for differentiating the rhythms of prose and poetry. The removal of this support tends in the majority of cases to convert the rhythm of poetry into the rhythm of prose, and vice versa.

(*c*) The rhythmic instinct is an affection of sensibility, varying to some extent with individuals. It has two aspects—motor and sensory. The test concerns only the former—the rhythm felt by the subject in uttering and not in hearing uttered a series of words. As a motor phenomenon the instinctive perception of the rhythmical time sense is subject to variations in the immediacy of response and in the universality of manifestation. This is most obviously dependent upon the degree of development of the power of coördinating time-units.

We begin now the detailed examination of the quantitative determination of the various groups of recurrences. The verse intervals will most conveniently be dealt with first.

C. Verse intervals.

The term verse is used in this discussion as synonymous with a line of poetry. The verse interval signifies the interval of sound which constitutes a run-on or end-stopped line of poetry. It is the time required in speaking to pass from the first to the last sound of the line.

The measurements are made from the beginning of the first to the end of the last sound of the verse. The pauses at the end of the verses are not included in the measurements. Hence the determinations yield a measure of the length and inequality of the verses only.

TABLE VIII.

(*Poetry.*)

Subject.	a	p	$\frac{p}{a}$	e	c	n
J. W. R.	2.20	0.36	0.15	+3.18 —1.56	+	32
E. W. S.	1.15	0.09	0.07	+1.30 —0.89	—	36
A. D. B.$_A$	2.41	0.30	0.12	+3.14 —1.91	+	6
W. W.	2.95	0.45	0.15	+4.04 —1.87	+	18
A. R. P.	1.63	0.21	0.12	+1.92 —1.35	+	4
H. Ö.	2.05	0.12	0.05	+2.25 —1.67	—	8
O. S.	1.87	0.12	0.06	+2.12 —1.53	—	7
Av.	2.05	0.23	0.10			

TABLE IX.

(*Poetry read as prose.*)

Subject.	a	p	$\frac{p}{a}$	c	n
J. M. T.	4.14	0.50	0.12	+	5
C. O.	3.92	0.30	0.07	+	5
W. C.	1.74	0.13	0.07	—	8
S.	1.66	0.12	0.07	—	8
Av.	2.86	0.26	0.08		

TABLE X.

(*Poetry read as poetry.*)

Subject.	a	p	$\frac{p}{a}$	c	n
J. M. T.	4.62	0.55	0.11	+	5
C. O.	3.84	0.24	0.06	+	5
W. C.	1.92	0.23	0.12	—	8
S.	2.39	0.26	0.10	+	8
Av.	3.18	0.32	0.09		

TABLE XI.

(*Prose read as poetry.*)

Subject.	*a*	*p*	$\frac{p}{a}$	*c*	*n*
S. I.	2.85	0.27	0.09	+	4
O. E. S.	3.53	0.31	0.08	—	4
Av.	3.16	0.29	0.08		

TABLE XII.

(*Summary of* TABLES VIII., IX., *and* X., *poetry.*)

Table.	*a*	*p*	$\frac{p}{a}$
VIII.	2.05	0.23	0.10
IX.	2.86	0.26	0.08
X.	3.18	0.32	0.09
Av.	2.69	0.27	0.09

Unit of measurement, 1^s.
a, average duration.
p, immediate probable error.
$\frac{p}{a}$, relative immediate probable error.
e, extreme intervals.
c, character of greater extreme.

These observations are based upon a study of 163 verses, various as to length and structure.

Extreme intervals.—The longest interval among the 111 verses is 5.01^s, 4th verse, record of J. M. T.; the shortest 0.89^s, Table VIII., E. W. S.; a range of 4.12^s. This is applicable to verse intervals, similarly composed, in general, presupposing that the figures fairly well represent the extremes.

Character of extreme intervals.—Column *c* of Tables VIII., IX. and X. furnishes the following data bearing upon the nature of the largest extreme deviation among individual verses: 60 per cent. are extremes of excess, 40 per cent. of deficiency, and 0 per cent. of equality. The tendency is thus to cause the longest individual deviation to vary more than the shortest from the length of the average.

Average-duration.—(1) *For the series.* The average for the series of 163 intervals is 2.69^s. For the series of records of prose read as poetry it is 3.16^s.

(2) *For the sets.* The shortest average of any set of records is 2.05^s, Table VIII., various types of scansion; the longest, 3.18^s, Table X., poetry read as poetry, making a range of 1.13^s for the average verse intervals of the different sets of records.

The average in the set of records where poetry was read as poetry is 0.32^s longer than in the records where poetry was read as prose.

The average in the set of records where prose was read as poetry is

0.04^s longer than the average of Tables IX. and X. (test records), and 0.11^s longer than in the set of records of Table VIII. (poetry).

(3) *For the records.*—The longest average for any record is 4.62^s, J. M. T., Table X.; the shortest, 1.15^s, E. W. S., Table VIII.; a range of 3.47^s. These figures presumably represent the time limits within which the average verse interval, similarly composed, of any reciter will fall.

The range for different records appears to be nearly three times larger than the range for different sets of records; and the range of the individual verses is about 1.29 times the range existing between the average intervals of different records.

Relative inequality.—(1) *For the series.* The inequality for the series of poetry is 9 % of the average duration of the intervals, and for prose, 8 %.

(2) *For the sets.*—The highest percentage is 10 %, Table VIII., types of scanned poetry; the lowest, 8 %, poetry read as prose, and prose read as poetry; a range of only 2 %.

(3) *For the records.*—The highest percentage among the individual records is 15 %, W. W. and J. W. R; the lowest, 5 %. This gives a range at 10 % for different records. Every subject was slightly more regular in reading the poetry as prose than as poetry. The highest difference between the regularity of the two records of the same subject is 0.05 (W. C.); the lowest, .01 (J. M. T. and C. O.).

Importance attaching to the duration of the verse interval.—The verse is the longest interval with which these measurements are concerned; hence the relevancy of the following remarks.

The interest that attaches to the length of the verse-group arises chiefly from a consideration of the span of the rhythmic time sense. How long a period is consciousness capable of rhythmizing? How short or how long must the unit be to enable the mind to coordinate it with other units of the same kind?

Gurney[1] holds that the most highly developed rhythmic sense can take in no more than a "small group"; and Sievers[2] and Schmidt,[3] equally vague, "strictly limit" the length of the sentence so that the time occupied in reading it may constitute a rhythmical whole. Bolton[4] concludes from experiments on subjective rhythmisation that there is a correspondence between the average time of a rhythmical unit, no matter

[1] Gurney, The Power of Sound, 127, London 1880.

[2] Sievers, Grundzüge der Phonetik, 4. Aufl., 218, Leipzig 1893.

[3] Schmidt, Introduction to the Rhythmic and Metric of the Classical Languages, 79ff., Boston 1878.

[4] Bolton, *Rhythm*, Am. Jour. Psych., VI 145–238.

what the form, and the normal wave of attention. The rate of speed determined the form, or the number of the components (8, 6, 4, or 2), of the group. No matter what the number, within certain limits, of the impressions, the duration of the group remained relatively the same, about 1^s. He gives 1.5^s as the upper limit.

It seemed that the natural period of attention exercised a regulative influence upon, and definitely limited the span of, the groups.

LANGE[1] found that the periodicities of the maxima and minima of attention for acoustic sensations varied from 3.5 to 4.0^s; for optic, from 3.0 to 3.4^s; and for electrical cutaneous, from 2.5 to 3.0^s.

STEVENS[2] posits a time interval between 0.53^s and 0.87^s, which is capable of being reproduced with the greatest accuracy. A plus interval results in a plus error; and a minus in a minus error. JOHNSON,[3] however, experimenting on practice and habit, concludes that there is no "indifference point" from which variations do not occur.

DIETZE[4] reports that the subject of his experiments in rhythm was incapable of grouping the impressions after the period had reached 4.25^s.

Other experiments have given the following results: for the MASSON disc, an average period of[5] 3.5^s (with average variation of 0.3^s), and of about 5 or 6^s[6] (with possible variation from 6 to 27^s[7]), the oscillation having been found most rapid[8] with a certain difference between the grayness of the disc and the ring; for voluntary effort,[9] a period of three-fourths of a second, attended by a large fluctuation and recovery; and for subjective rhythmisation of sounds, the "lower limit"[10] of the intervals has been placed at less than 2^s; others give a limit of less than 0.4^s[11] and 0.5^s[12], and of more than 1.0^s.[13]

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[1] LANGE, *Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen apperception*, Philos. Studien, 1888 IV 404.

[2] STEVENS, *On the time sense*, Mind, 1886 X 394

[3] JOHNSON, *Researches in practice and habit*, Stud. Yale Psych. Lab., 1898 VI 91.

[4] DIETZE, *Untersuchungen über den Umfang des Bewusstseins bei regelmässig aufeinanderfolgenden Schalleindrücken*, Philos. Studien, 1885 II 383.

[5] PACE, *Zur Frage der Schwankungen der Aufmerksamkeit nach Versuchen mit Masson'schen Scheiben*, Philos. Studien, 1893 VIII 394.

[5] TITCHENER, A Primer of Psychology, 89, New York 1898.

[7] TITCHENER, An Outline of Psychology, 141, New York 1896.

[8] MARBE, *Die Schwankungen der Gesichtsempfindungen*, Philos. Studien, 1893 VIII 615.

[9] SCRIPTURE, The New Psychology, 125, New York 1897.

[10] SMITH, Philos. Studien, 1900 XVI 282.

[11] MEUMANN, Philos. Studien, 1894 X 302.

[12] MARTIUS, Philos. Studien, 1891 VI 196.

[13] TITCHENER, Experimental Psychology, I, ii 340, New York 1901.

These figures indicate: (1) That, while the differences in the results of experiments may be partly explained by differences and inconstancy in the amount of the stimuli and in the physical and mental conditions of the observers (due to fatigue, inattention, irrelevant movements, sensations and memory images, passive or active attitude, etc.), the natural period of attention will differ for the different senses and for different individuals. The period for the more purely sensory (receptive) waves may also differ from the more purely motor (voluntary) waves; and those of involuntary subjective grouping from those of voluntary grouping.

(2) That the span of the rhythmic sense is definitely limited for each individual, though we do not yet know its precise upper or lower limits, nor how it differs in voluntary and involuntary subjective rhythmisation, in its sensory and motor aspects, nor its dependence on the psychophysical conditions and age of the individual.

(3) That the regularity of intervals will be greater for those that fall within the span than for those that do not. This will be illustrated presently.

Rhythmical signification of irregularity of intervals.—The definition of rhythm entails a limiting conception, as regards both the span and regularity of intervals. What does the concept of rhythm require? Absolute or relative conformity to a standard? How great a variation may intervals admit of without becoming unrhythmical, or non-periodic?

A series of intervals of an average duration of 4.62^s (J.M.T.) manifestly will admit of more absolute variation than one of 1.15^s (E.W.S.). Hence the amount of variation expressed as a percentage of the average duration (the relative immediate probable error) yields the index of regularity. How small must this be to preserve the rhythmical unity of intervals?

This question emphasizes the distinction between hap-hazard recurrences and strict periodicities—between coordinated (that is, rhythmical) sequences, and uncoordinated (that is, unrhythmical) sequences. It is a mistake to subsume all recurrences[1] of maxima and minima, all repetitious undulations and oscillations, under the concept of rhythm, and treat literary, social, geological, *et al.*, phenomena as rhythmical. Prior to legitimately making such sweeping subsumptions a measure or limit to the irregularity for any series of occurrences must first be adopted. This involves the adoption of a *criterion* of *irregularity*.

It has been established, by facts admitting of profuse illustration,[2] that things come and go in waves and periods; that all motion is oscillatory

[1] SPENCER, First Principles, X, The Rhythm of Motion, New York 1883.

[2] FISKE, Outlines of Cosmic Philosophy, II.

and not uniform, either in the sense of continuing at precisely the same rate of motion, or invariably in the same direction. But undulations that are not well coordinated periodicities are mere recurrences.

The limit of irregularity cannot be fixed *a priori*. It may be determined by taking an easily rhythmisable series of intervals and increasing the inequality until the subject is incapable of rhythmising them. This practical test is based upon the fact that the feeling of rhythm is non-existent for me, until it becomes an affection of my sensibility. The only rhythm which can become an object of direct experience is that of a succession of psychoses. Hence the ultimate criterion is to be found in the sensibility of beings rhythmically constituted.

In the present investigation the limits may be approximately determined in units of time by ascertaining the average amount of inequality of those records the intervals of which the ear is incapable of coordinating.

Each record of Table VIII. was listened to a number of times, and the attempt made to coordinate the sequences of impressions representing the verses. The reproducing speed was relatively the same as the recording speed. Appeal was then made, where the rhythm was obscure, to the motor aspect of the rhythmical sense.

With reference to the more purely motor aspect of the attempted grouping, it was difficult to impart a very distinct unity to the verses of the J. W. R. and A. D. B. records, unless the lines were repeated rapidly and the terminal pauses were considerably prolonged. The rhythm then became marked. The verses of the W. W. record with this device, were capable of being only vaguely coordinated and grasped as totalities. These verses contain twelve syllables.

Listening to the records on the phonograph, it was difficult to coordinate, and grasp as rhythmical wholes, several of the verses of the J. W. R., and most of the W. W. record. In the A. D. B. records it was possible to feel a certain verse rhythm; in the A. R. P. record, it was more distinct; in the three remaining records it was marked, especially in the H. Ö. and in the last stanza of the E. W. S. record. In general, other things being equal, the shorter verses were more unitary and rhythmical.

Bearing upon the limit of inequality and span of the rhythmic sensibility.—These results suggest that when the deviation rises to a fraction of 15% of the length of the whole interval, the mind finds it hard to rhythmise the impressions; when the fraction is 12% the rhythm is still a little vague; and when it is 7% the intervals are easily coordinated. When the fraction is smaller, of course, the rhythm becomes increasingly distinct.

As regards units of time, the rhythm of the verse intervals was almost

destroyed when the deviation reached a third of a second; it was somewhat disturbed when it reached a fifth of a second; and when it was about one-tenth of a second, it was well preserved.

From the measurements of the irregularity of the verse intervals, it is difficult to determine the maximum length of an interval that can be synthesized into a rhythmical whole. The longest span where the rhythm was felt, though vaguely, was 2.41^s; except in the Persian record, where the line meter rhythm was very distinctly felt and the intervals averaged 2.45^s. Here the absolute irregularity and relative irregularity reached the low figures of 0.06^s and 0.02 respectively. That the Persian line group should manifest a relatively high degree of regularity was to be expected *apriori* from the Persian theory of poetry. In Japanese poetry, which theoretically follows the same law with respect to its meter, the irregularity is 0.06^s for J. K. (av. length of verses = 0.87^s) and 0.09^s for I. M. (av. = 0.95^s), and the fraction is 0.07 and 0.09 respectively. While, therefore, the verse meter of both these languages conforms with the requirements, as respects degree of regularity, of rhythm, the records show that English poetry, which theoretically does not recognize the verse meter, except as secondary, affords specimens which surpass the Japanese in regularity and which, if we include the record of mechanical scansion without regulative concomitant where the figures are 0.06^s and 0.03, are practically equal to the Persian. No other intervals in the records of the oriental languages are equal to the regularity of the verse intervals. (In the Persian record the expiration and the verse intervals coincide.) In English the simple centroid intervals are frequently as regular.

The *long* limit of the rhythmic sense for verse intervals, themselves containing minor intervals (rhythms), probably does not generally exceed two and a half seconds. Perhaps we may say that those of less than one and three-fourths of a second in length (with inter-verse pauses of average length) give for most subjects the best results rhythmically (1.67^s = average length of verses in the most rhythmical records, viz., E. W. S., H. Ö., O. S., A. R. P., J. K., I. M. and K. H. K.). This figure is considerably above MEUMANN's, MARTIUS's and TITCHENER's results, and slightly below SMITH's, for subjective accentuation; considerably above SCRIPTURE's results for volitions, and below PACE's, LANGE's and TITCHENER's results for the fluctuation of sensations. Hence the results of this experiment may apply only to verse intervals. This span should not be identified offhand with the spans *(a)* between the oscillations of attention for sensations, memory-images, illusions and simple voluntary efforts, and *(b)* between the accents in voluntary and involuntary subjective rhythmisation.

Results.—The verse interval rarely exceeds 5^s in length, nor does it fall much below 0.80^s (a range of a little over 4^s).

About 60 per cent. of the verse intervals that deviate most from the length of the average are extremes of excess. The balance are extremes of deficiency.

The average duration of a verse interval is about 2.69^s. It varies according to the character of the line and will lie for a given number of reciters, somewhere between approximately 2.05^s and 4.62^s, a range of 3.47^s.

In units of time the verse intervals of a given number of recited verses will vary from the length of the average by about 0.27^s.

The range of irregularity existing between a given number of recited verses of a given number of subjects may be approximately 0.46^s (from 0.09^s to 0.55^s).

As a fraction of the average, the irregularity of a given number of recited verses of a given number of subjects will be about 9%; and the range about 10% (from 5% to 15%).

The regularity is never precisely the same for different records of the same person. The difference will rise to about 5%.

Whenever the amount of irregularity exceeds 15 per cent. of the average duration, the mind finds it hard to rhythmise the series. The rhythm is obscure. Apparently when the percentage does not rise above 10%, the rhythm is fairly well preserved.

If we accept, provisionally, 10 per cent. of deviation as the limit of allowable inequality in good rhythm, 59 per cent. of the records of verse intervals are rhythmical and 41 per cent. are not.

The nature of the unity of the verse interval.—Assuming that a spoken line of poetry does not exceed the limits of the grasp of the rhythmic sense, and that it is rhythmically coordinated with other lines, it will stand forth as a unity in consciousness. Every verse is so far forth an absolute unity, whether its components are homogeneous or not, *i. e.*, made up entirely of sounds, or predominantly of sounds and partly of pauses. Psychologically a rhythmical line is a mental synthesis, regardless of its structure.

As a physical event, however, to constitute a unity the elements of speech must make an uninterrupted continuum. The unity of a verse is coterminous with homogeneity of process. The sounds constitute one order of homogeneity; the pauses, another. Whenever the two intermingle the spoken verse physically becomes heterogeneous, *i. e.*, non-unitary. Hence to be a physical, or physiological, unity, a verse must contain no sectional pauses. Such a verse unity, however, would, if

frequently repeated, become thread-bare and monotonous, although it would result in the best quality of rhythm.

Examples of verse intervals, that are physically homogeneous unities, may be seen by referring to the I. M., J. K., K. H. K., E. W. S. and H. Ö. records of V., B, above.

A physically homogeneous unity will coincide with a psychological unity when its limits are so coordinated as to coincide with the rhythmic beats of the latter.

The principles of the rhythmical coordination of verse intervals.—The principle according to which the records were differentiated as rhythmical and non-rhythmical, was that of time-coordination. The intervals formed a series of rhythmical intervals only provided the law of rhythm, as regards amount of inequality and length of span was observed. This principle, however, was found incapable of comprehending a complete explanation of the totality of the experience. To account fully for all cases of verse rhythm, it is necessary to invoke another principle,—that of emphasis. This is best exemplified in sing-song.

6. *Sing-song scansion.*—The table of measurements justifies the belief that the verses of the O. S. record (reading scansion) should give rise to the most distinct feeling of rhythmical unities. This is counter to the judgment of the ear, however, which found it easiest to grasp and retain in consciousness as rhythmical unities the first three verses, respectively, of the two stanzas of the H. Ö. record, and the last stanza of the E. W. S. record. These appeared completely unified sequences, not because the coordination impressed the ear as perfect, but because the principle of emphasis rendered the unity distinct in consciousness.

This unifying principle is more or less operative in all the records, but in the record of sing-song it follows a unique law, which may be expressed as an exact correspondence line for line, of certain centroids, as regards distribution and degree of intensity. When the major centroids are correlated vertically,—that is, placed on the numerically correspondent syllables of each line—the type of scansion differs from reading, doggerel and routine scansion, which at their best, merely attain a maximum regularity, in its strict observance of this law of emphasis. This species of scansion we may designate sing-song. It is this which produces the most vivid impression of verse unities. No verse rhythm can, psychologically considered, be compared with the sing-song.

To illustrate: in the E. W. S. record (last stanza), the beginning of every line received a specially strong accent upon the corresponding syllables. The accentuation was uniformly of the third degree, except in the last line. The stress of the last syllables of the verses, was uniformly sub-

ordinated to the stress of the first syllables. The intervening syllables, with one exception, were subordinate to those at the end. The predominant elements of the centroids were intensity for those at the close, and pitch and intensity for those at the beginning of the lines.

In the H. Ö. record the same general law is exhibited. The second degree centroid was placed, however, on the last syllable but one. This did not affect the sing-song swing. This record also differed in having a sectional pause between the two chief centroids. In both, however, the pauses at the end of the verses served to support the unity of the lines.

Conclusions.—These are suggested by the foregoing. Sing-song scansion is peculiar in being largely a matter of verse interval rhythm.

This peculiarity consists (a) in paying a strict regard to the temporal coordination of the intervals and (b) in observing a law of distribution and subordination of centroids. In respect to distribution, the law seems to require two strong centroids to the verse, placed on the corresponding syllables of each line, the one near the beginning, the other near the close. Hence in the distribution of the sing-song centroids, there is a balanced line arrangement.

In respect to subordination, there is a like correlated arrangement, line for line. But in each line, the centroids are unevenly balanced. It seems to open with a very strong explosion of the cords in which the element of pitch is prominent, is followed by a weak one and ends up with a fairly strong explosion in which the element of pitch is less prominent. For different speakers the distribution of the second and third degree centroids may be reversed.

It has been customary to explain the peculiarity of sing-song scansion as merely a phenomenon of exact rhythm.[1] To fully account for it as an occurrence in consciousness, however, a twofold principle needs be invoked.

Logical or reading scansion and doggerel and routine scansion observe no such principle, and the rhythm of the centroid intervals (cf. V., F., below) in reading scansion is almost invariably, and in doggerel scansion generally, less regular. Routine has the presumption of greater regularity than either doggerel or sing-song scansion, as may be seen by a comparison of the records.

Sing-song scansion might figuratively be called *pendulum-swing* scansion, where the height of the two swings differ somewhat, and routine or mechanical scansion, metronome scansion, where the strength or reach of the two swings is equal. In the latter kind, where the motive is merely to rigorously coordinate the distances between centroids, the function of

[1] ELLIS, The Quantitative Pronunciation of Latin, London 1874.

the sectional and terminal pauses becomes compensatory. They must be strictly limited.

Doggerel scansion is the most nearly like reading scansion, which is rhythmically free. The latter differs from the former in that it pays more heed to the logic or thought contained in the verses. When the logical and metrical accents coincide it tends to become doggerel. The two often coincide, as is instanced by the first six stanzas of the E. W. S., and the stanza of the A. R. P. records.

D. *Expiration (or sound) intervals.*

1. *Explanation.*—SWEET's statement that the only divisions which are actually made in language are those into "breath groups," each group forming a closed system, in which, contrary to general opinion, there is no pause, as it stands, vague and unqualified, must be rejected. It does, however, express an important truth and may, with proper limitations, be accepted.

It is obvious that speech, as a physiological process, cannot continue indefinitely so as to form a perfect continuum. The vocal cords are incapacitated from functioning in an unbroken continuity, partly because such activity is antagonistic to the necessary process of alternate waste and repair, partly because the capacity of the air supply in the lungs is definitely limited for each individual, and partly because the specific function of speech, the expression of thought, could not be thus subserved. Hence the need of making divisions into sounds and pauses is inescapable.

In the sense, therefore, of a succession of physical or physiological processes in time, the only speech divisions are those into filled and vacant groups. These represent periods of expiration and inhalation. While physiologically they are indisputably the only divisions of speech, psychologically, other divisions obtain.

It is both true and false that within each expiration group no gaps occur. In the sense in which the word silence is used on p. 35 above, it is false. It has been shown that practically every sound is separated from every other by a gap, which, however, is generally so minute as to be imperceptible by the naked ear. The average duration is only 0.10^s. Hence, if by pauses is not meant the necessary silences which intervene between separate sounds, but those gaps which, in the main purposeful, separate groups of words by silences other than those involved in the mere production of a series of sounds, it is true that the intervals in question constitute a closed system.

By expiration interval, then, is indicated a sequence of sounds limited by pauses. The following tables give the results of the measurement of

these intervals. The measurements extend through the entire length of the sequences. Only the lengths of the different sound intervals are given: not the sound intervals and pauses taken together. Every sound interval is invariably convertible into an expiration interval, since the outgoing of breath is the indispensable condition of producing vocal sounds. The pause intervals, contrariwise, are not invariably convertible into inhalation intervals. It requires less time to inhale a quantity of air than it takes to consume it in speaking. And oftentimes the quantity inhaled is so oppressive or excessive as to require exhaling during the pause interval, as is frequently seen in persons of a nervous disposition.

2. *Tables of measurements of expiration intervals.*

TABLE XIII.

(*Poetry.*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. W. R.	2.60	0.98	0.37	+ 4.77 — 0.48	+	27
E. W. S.	1.18	0.22	0.18	+ 2.31 — 0.41	+	35
A. D. B.$_A$	0.75	0.28	0.37	+ 1.73 — 0.14	+	15
A. D. B.$_B$	1.00	0.37	0.37	+ 2.16 — 0.25	+	8
W. W.	1.06	0.37	0.34	+ 2.62 — 0.18	+	38
A. R. P.	1.26	0.45	0.35	+ 1.90 — 0.12	—	5
J. K.	2.15	0.35	0.16		0	2
I. M.	2.39	0.33	0.13		0	2
K. H. K.	2.45	0.06	0.02	+ 2.54 — 2.31	—	4
H. Ö.	1.41	0.30	0.21	+ 2.02 — 0.65	—	11
O. S.	1.87	0.12	0.06	+ 2.12 — 1.53	—	7
E. H. T.$_A$	1.72	0.87	0.50	+ 3.45 — 0.54	+	4
E. H. T.$_B$	1.68	0.13	0.07	+ 2.00 — 1.56	+	4
Av.	1.65	0.37	0.24			

TABLE XIV.

(*Prose.*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
G. A. A.	1.37	0.42	0.30	+ 3.00 — 0.32	+	21
W. L. P.	1.25	0.19	0.15	+ 1.90 — 0.12	+	20
C. O. S.	1.50	0.72	0.48	+ 3.42 — 0.15	+	21
Av.	1.37	0.44	0.31			

TABLE XV.

(*Poetry read as prose.*)

Subject.	a	p	$\frac{p}{a}$	e	c	n
C. O.	0.93	0.36	0.38	+ 1.60 — 0.12	—	17
W. C.	1.09	0.37	0.33	+ 2.50 — 0.70	+	12
S.	1.12	0.39	0.34	+ 2.10 — 0.32	+	12
Av.	1.04	0.37	0.35			

TABLE XVI.

(*Poetry read as poetry.*)

Subject.	a	p	$\frac{p}{a}$	e	c	n
C. O.	0.85	0.35	0.41	+ 1.85 — 0.12	+	17
W. C.	0.90	0.32	0.35	+ 1.85 — 0.27	+	15
S.	1.08	0.50	0.40	+ 3.00 — 0.25	+	15
Av.	0.94	0.39	0.40			

TABLE XVII.

(*Poetry read as prose without punctuation marks.*)

Subject.	a	p	$\frac{p}{a}$	e	c	n
G. F. A.	1.01	0.37	0.36	+ 2.00 — 0.22	—	16
B. S. G.	0.90	0.30	0.33	+ 2.07 — 0.25	+	16
Av.	0.95	0.33	0.34			

TABLE XVIII.

(*Prose read as poetry.*)

Subject.	a	p	$\frac{p}{a}$	e	c	n
S. I.	1.07	0.34	0.31	+ 2.25 — 0.60	+	9
O. E. S.	1.40	0.66	0.47	+ 3.46 — 0.27	+	9
Av.	1.23	0.50	0.39			

TABLE XIX.

(*Table of summaries of expiration intervals of speech.*)

Tables.	a	p	$\frac{p}{a}$
XIII.	1.65	0.37	0.24
XIV.	1.37	0.44	0.31
XV.	1.04	0.37	0.35

XVI.	0.94	0.39	0.40
XVII.	0.95	0.33	0.34
XVIII.	1.23	0.50	0.39
Av.	1.19	0.40	0.34

Unit of measurement, 1^s; *a*, average duration; *p*, immediate probable error; $\frac{p}{a}$, relative immediate probable error; *e*, extreme intervals; *c*, character of greater extreme.

3. *Observations.*—The study is based on 362 measurements.

Extreme intervals.—Of the 362 intervals the longest is 4.77^s; the shortest, 0.12^s; a range of 4.65^s.

Character of greater extreme.—72 per cent. of the intervals which deviate the farthest from the average are extremes of excess, 19 per cent. of deficiency, and 8 per cent. are equal. The tendency in about three-fourths of the cases is to lengthen unduly, rather than shorten, the expiration intervals.

Average duration—(1) *For the series.* The average for the 362 intervals is 1.19^s.

(2) *For the sets.* The longest average of any set of records is 1.65^s. Table XIII., poetry; the shortest, 0.94^s, Table XVI., poetry read as poetry. This gives a range of 0.71^s.

The average of the set of prose records, Table XIV., is 1.37^s. This is 0.28^s less than for the corresponding set of records of poetry, Table XIII.

The set for poetry read as prose without punctuation marks, Table XVIII., affords the second shortest interval, 0.95^s. It is 0.01^s longer than for poetry read as poetry. The number of intervals is about the same in both tables.

The reading of the poetical passage as prose, Table XV., test records, resulted in lengthening the interval. The difference is 0.10^s. This is in disagreement with the result noted above, that the expiration interval in reading poetry is longer than in reading prose. The discrepancy may be explained by the circumstance that the passage when read as prose was unfamiliar to the subject. This would entail a retarded utterance.

The intervals of the test records where prose was read as poetry, Table XVIII., are longer than in any of the other three test records.

(3) *For the records.*—The highest average duration of any single record is 2.60^s, J. W. R., Table XIII., reading scansion; the lowest, 0.75^s; Table XIII., A. D. $B._A$, poetry; a range of 1.85^s. The range for the records is 2.60 times larger than for the sets; and the range for the individual intervals is 2.51 times larger than for the records.

The greatest difference between the averages of two different records of the same subject is 0.19^s; the smallest, 0.04^s; a range of 0.15^s.

Regularity—(1) *For the series.* The average inequality for the series of records is 0.40^s. This is 34% of length of the average.

(2) *For the sets.* The highest degree of irregularity is 40%, Table XVI., poetry read as poetry; the lowest, 24%, Table XIII., different types of scansion of poetry; a range of 16%.

In units of time, the greatest irregularity is 0.50^s (Table XVIII.); the lowest, 0.33^s (Table XVII.); a range of 0.17^s.

The irregularity was 5% smaller in the records where poetry was read as prose than in the records of the same passages read as poetry. The regularity for poetry read as prose without punctuation marks is greater than either; it is unexpectedly high.

The regularity for the records of scanned poetry is noticeably greater than for any other set.

(3) *For the records.*—The greatest absolute irregularity of any individual record, J. W. R., Table XIII., poetry, is 0.98^s, or over one-third of the average length of the intervals; the lowest, ignoring the Persian record where the verse and expiration groups are coincident, is 0.12^s, O. S., Table XIII., routine scansion; a range of 0.86^s. This is five times the range existing between the sets of records.

The highest amount of relative irregularity is 50%; the lowest 6%; a range of 44%. This range is 2¾ times the range existing between the sets.

In the selections of the test read as prose and poetry, the amount of irregularity is higher, both absolutely and relatively, for the poetic rendering. This is at variance with Tables XIII. and XIV. A like discrepancy was noted above between the set of test records and those of prose and poetry. The discrepancy calls for explanation. May we assume the existence of a special talent for reading poetry, distinct from the ability to properly read prose? Some persons discourse eloquently on scientific themes, but make a botch of reciting poetry.

The greatest difference in the different records recited by the same person is 6%; the smallest, 2%; a range of 4%, which is exceedingly small.

4. *Results.*—The length of the expiration interval of ordinary speech may be presumed to lie somewhere within the limits of about 0.12^s and 4.77^s. In impassioned oratory the upper limit will evidently be overreached; the lower limit is relatively fixed (average unemphatic syllable $= 0.11^s$).

About 72 per cent. of the expiration intervals of speech which deviate the farthest from the normal or average interval are extremes of excess; about 19 per cent., of deficiency; and about 8 per cent. are equal.

The length of the average expiration interval of speech will be approximately 1 [illegible].

The difference in the length of the average expiration intervals of ordinary speech of different speakers is limited to about 1.85^s.

As between the length of the average expiration interval of prose and poetry, the evidence suggests that the interval is slightly longer in the declamation of poetry. The explanation suggested is, (*a*) that the intervals in poetry tend to become unities coterminous with the verses (the average verse interval is about 2.26 times longer than the average expiration interval); and (*b*) that the more lyrical and impetuous emotions of poetic diction result in prolonging the intervals. The latter circumstance should obtain equally in impassioned prose.

The average length for the same person reciting the same lines on different occasions is not the same. The difference varies roughly from 0.04^s (smallest difference) to 0.19^s (largest difference).

The inequality in the length of successive expiration intervals will be equal to about one-third of the length of the average.

In units of time, the range in the inequality of the expiration intervals of different speakers will be about 0.86^s (from 0.12^s to 0.98^s).

As a fraction of the average, the range of the inequality for different speakers will be about 44% (from 6% to 50%).

The regularity for the same person reciting the same passages on different occasions is not the same. It varies from something like 2% to 6%.

As between prose and poetry, the evidence suggests that the regularity is slightly higher for the latter.

The elimination of punctuation marks does not seem to increase the irregularity.

Accepting provisionally 10 per cent. as the criterion, the coordination of the lengths of expiration intervals of speech is rhythmical in 12 per cent. of the records.

5. *Interpretation of results.*—What is the psychological import of the above results? With what laws of mental precedure can, for example, the brevity of the expiration group be correlated? If we accept as true the doctrine of the periodic character of attention,[1] we find here a fitting illustration of its applicability to speech. Attention, subjectively considered, is detention in consciousness of sensations and other psychical phenomena that, through special fixation, have been exalted to a high degree of intensity, vividness and completeness. But this process of fixating the mind is of the nature of a wave or pulsation: it consists in an alternation of max-

[1] LADD, Psychology, Descriptive and Explanatory, 71 fl, New York 1896.

imal and minimal efforts. The period of these attentive efforts has been variously given (*cf.* VI., C, 3, above). The average sound group easily falls within the extremes. Manifestly, were the laws of attention disregarded in the expiration intervals of speech, the result would be a special strain upon the power of attention, resulting in a sacrifice of interest, in fatigue and diminished mental grasp. The laws of mental activity seem to demand that the words in speaking be grouped into short unities that agree, in the main, with the "unity of consciousness"; and that frequent, though brief, pauses be made to enable the mind to easily grasp and synthesize this manifold of sensation.

All this is important as affects the doctrine of conservation of psychical —not speaking now of physiological—energy. The mind conserves its energy by dividing its work into brief, but frequent and forceful, efforts, and by introducing brief and frequent, rather than long and infrequent, rests. This demand is unconsciously fulfilled in spoken language. The frequent pauses afford momentary opportunities for rest for both speaker and listener. Hence the possibility of listening attentively to a long discourse. A speaker who easily tires himself and his audience, and fails to rise to the full measure of his possibilities, will find here, it would seem, a suggestion.

It appears from our tables that the average duration of the intervals is less than our average wave of attention ($1\frac{3}{4}''$). This suggests an explanation of the phenomenon that the extremes of excess are vastly more numerous than the extremes of deficiency. On this supposition, we should expect to find, with an average that is rather in excess of the wave of attention, a greater number of minus extremes. The theory is suggestive, and is offered as an explanatory hypothesis that seems plausible and consistent with known laws of mental functioning.

The high degree of irregularity exhibited in the expiration intervals makes it evident that a supplementary principle is needed to complete the theory advanced above, namely the principle of change or variety. While the laws of mental behavior require the duration to be limited they do not require invariable uniformity of length within the limits. Such uniformity would result in intolerable monotony, and would be antagonistic of the essential rhythm of speech, the centroid rhythm. Hence the high degree of inequality displayed in the expiration intervals may conduce to restive and pleasing variety.

The numerical relation of syllables to the normal and maximal expiration intervals is shown in VI., 5, below.

E. *Vacant intervals.*

1. *Explanation.*—A vacant interval in speech is the sequence which intervenes between the adjacent termini of two consecutive expiration intervals. The interval is entirely void of sound. In contra distinction to the gaps which separate different components of a sound or expiration interval, these vacancies separate different sound intervals themselves. They may be called pauses, every variety of which occurring in uttered language is included in the term. In music the terms rest and stop are most frequently used.

The measurements extend from the end of the last or vanishing sound of the one interval, to the beginning of the first or appearing sound of the following interval. Only the length of successive pauses are compared. The rhythmic character of the alternation of vacant intervals and expiration intervals (roughly the rhythmic action of the lungs in speaking) can be obtained by taking both as unities of a series, finding the sum of the measurements of both, and the probable error, according to the formula on p. 39.

2. *Measurements of vacant intervals.*

TABLE XX.

(*Poetry.*)

Subject.	a	p	$\frac{p}{a}$	e	c	n
J. W. R.	0.63	0.19	0.30	$+1.10$ -0.18	+	25
E. W. S.	0.43	0.11	0.25	$+0.87$ -0.14	+	28
A. D. $B._A$	0.48	0.23	0.47	$+0.16$ -0.12	+	14
A. D. $B._B$	0.52	0.19	0.36	$+1.16$ -0.31	+	7
W. W.	0.57	0.15	0.26	$+1.06$ -0.16	+	37
A. R. P.	0.46	0.17	0.36	$+0.82$ $-0\ 20$	+	4
K. H. K.	0.54	0.06	0.11	$+0.66$ -0.45	+	3
H. Ö.	0.46	0.13	0.28	$+0.80$ -0.20	+	10
O. S.	0.44	0.09	0.20	$+0.62$ -0.28	+	7
E. H. $T._A$	0.21	0.01	0.04			3
E. H. $T._B$	0.21	0.017	0.08	$+0.25$ -0.18	+	3
Av.	0.45	0.12	0.24			

TABLE XXI.

(*Prose.*)

Subject.	a	p	$\frac{p}{a}$	e	c	n
G. A. A.	0.57	0.14	0.24	+1.17 —0.15	+	20
C. O. S.	0.40	0.16	0.40	+0.97 —0.17	+	19
W. L. P.	0.72	0.28	0.38	+1.45 —0.20	+	10
Av.	0.56	0.19	0.34			

TABLE XXII.

(*Poetry read as prose.*)

Subject.	a	p	$\frac{p}{a}$	e	c	n
J. M. T.	0.39	0.12	0.30	+0.60 —0.10	—	14
C. O.	0.43	0.13	0.30	+0.79 —0.12	—	16
W. C.	0.71	0.28	0.39	+1.25 —0.12	—	11
S.	0.67	0.22	0.32	+1.32 —0.12	+	11
Av.	0.55	0.18	0.32			

TABLE XXIII.

(*Poetry read as poetry.*)

Subject.	a	p	$\frac{p}{a}$	e	c	n
J. M. T.	0.62	0.15	0.24	+0.91 —0.16	—	14
C. O.	0.44	0.13	0.29	+0.73 —0.18	+	16
W. C.	0.68	0.22	0.32	+1.20 —0.22	+	12
S.	0.62	0.18	0.29	+1.12 —0.17	+	14
Av.	0.59	0.17	0.28			

TABLE XXIV.

(*Poetry read as prose without punctuation marks.*)

Subject.	a	p	$\frac{p}{a}$	e	c	n
J. M. T.	0.35	0.10	0.28	+0.60 —0.14	+	17
G. F. A.	0.47	0.15	0.31	+0.71 —0.17	+	15
B. S. G.	0.44	0.15	0.34	+0.87 —0.12	+	15
Av.	0.42	0.13	0.31			

TABLE XXV.
(*Prose read as poetry.*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
S. I.	0.40	0.14	0.35	+0.72 —0.17	+	8
O. E. S.	0.43	0.09	0.20	+0.65 —0.25	+	8
Av.	0.41	0.11	0.27			

TABLE XXVI.
(*Summaries.*)

Tables.	*a*	*p*	$\frac{p}{a}$
XX.	0.45	0.12	0.24
XXI.	0.56	0.19	0.34
XXII.	0.55	0.18	0.32
XXXIII.	0.59	0.17	0.28
XXIV.	0.42	0.13	0.31
XXV.	0.41	0.11	0.27
Av.	0.44	0.15	0.29

Unit of measurement, 1^s; *a*, average duration; *p*, immediate probable error; $\frac{p}{a}$, relative immediate probable error; *e*, extreme intervals; *c*, character of greater extreme; *n*, number of intervals.

3. *Observations.*—The tables afford a study of 238 pauses.

Extreme intervals.—The W. L. P. record, colloquial prose, Table XXI., contains the longest individual interval, of the entire series, 1.45^s: The J. M. T. record, Table XXII., poetry read as prose, the shortest, 0.10^s; a span of 1.35^s.

The second longest is 1.32^s, poetry read as prose. Where poetry was read as prose without punctuation marks, the length of the longest interval does not exceed 0.87^s. Several intervals occupy 0.12^s.

Doubtlessly the longest extreme, especially in conversation, is frequently exceeded. These measurements do not include stops, such, for example, as are made in lecturing to students taking notes, in collecting one's wits, in deliberating, etc.

Character of greater extreme.—88 per cent. of the extremes are +, 12 per cent. —, and 0 per cent. 0. (The first are extremes the difference between which and the average is greater than the difference between the average and the shortest extreme: the second are extremes the difference between which and the average is greater than the difference between the average and the longest extremes: in the third case the extremes of excess and deficiency are equal.)

Average duration.—(1) *For the series.*—The average length of the 238 pauses, Table XXVI., is 0.44^s.

(2) *For the sets.*—The highest average is 0.59^s, Table XXIII., poetry read as poetry, which is followed by 0.56^s, Table XXI., prose; the lowest, 0.41^s, Table XXV., prose read as poetry; a range of 0.18^s. The average for the set read without punctuation marks is 0.42^s.

The average for the table of poetry is 0.11^s less than for the table of prose, whereas in Table XXIII., poetry read as poetry, it is 0.04^s greater than in Table XXII., poetry read as prose. An examination of the four tables suggests that the discrepancy might be due to the range in the character of the records of Tables XX. and XXI. The former contains the shortest average interval, in the record of routine scansion: the latter, the longest, in the record of colloquial prose. Eliminating these extremes, the averages will very nearly coincide.

The duration of the pauses in the set of records of the passage read without punctuation marks is 0.08^s shorter than for the pauses of the same passages reads as prose and poetry with the punctuation marks. The average number of pauses for the former set, however, is 15.6, for the latter, 13.5, a difference of 2.1. The highest number in any one record of the sets, 17, is contained in the Browning passage without the marks: the lowest number, 11, in the Tennyson passage read as prose with the marks. In the records of the same passage (Tennyson) without the marks the number is 15.

(3) *For the records.*—The highest average for any individual record is 0.72^s, W. P. L., Table XXI., prose, which is followed by 0.71^s, W. C., Table XXII., poetry read as prose, and 0.68^s, W. C., Table XXIII., poetry read as prose; the lowest is 0.21^s, E. H. T.A and B., Table XX., routine scansion, which is followed by 0.35^s, J. M. T., Table XXIV., poetry read as prose without punctuation marks; a range of 0.51^s. The range for the records is 2.73 times the range for the sets: and the range for the individual intervals is 2.64 times the range for the average intervals of the records. In the passages that were read by the same subject on two different occasions the highest difference is 0.23^s (J. M. T.); the lowest, 0.01^s (C. O.); a range of 0.22^s.

Absolute irregularity.—(1) *For the series.*—The irregularity in the coordination of the duration of the intervals of the series, Table XXVI., is 0.15^s.

(2) *For the sets.*—The highest amount is 0.19^s, Table XXI., prose; the lowest, 0.11^s; Table XXV., prose read as poetry; a range of 0.08^s.

The average for Table XXIII., poetry read as poetry, is 0.01^s smaller than for Table XXII., the same passages read as prose. Table XXIV., where the punctuation marks were left out, is more regular than either.

The poetry records, Table XX., are 0.07^s more regular than the prose records, Table XXI.

(3) *For the records.*—The highest is 0.28^s, W. L. P., Table XXI., prose, and W. C., Table XXII., poetry read as prose; the lowest, 0.01^s, Table XX., E. H. T.A, routine scansion; a range of 0.27^s. This is approximately three and one-half times the range for the sets.

Relative irregularity.—(1) *For the series.* The average is 29%. Table XXVI.

(2) *For the sets.*—Table XXI., prose, contains the highest percentage of irregularity, 34%; Table XX., different types of scansion, the lowest, 24%; a range of 10%.

The inequality for the set where poetry was read as poetry, Table XXIII., is 4% smaller than for the sets where poetry was read as prose. The reading of the passages without the punctuation marks is 1% more regular than the former. The pauses in Table XX., poetry, are 10% more regular than in Table XXI., prose.

(3) *For the records.*—The largest amount of irregularity in any record is 47%, A. D. B., Table XX.; the smallest, 4%; making the extent of the range 43%. This is 4.30 times the range existing between the different sets of speech pauses.

The largest difference in the regularity of the pauses of the different records of the same person is 7%, W. C., the lowest, 1%, C. O.; a range of 6%.

The irregularity in the Tennyson stanzas of Tables XXII. and XIII., is greater than in the Browning stanza. In Table XXII. the difference is 5%, and in Table XXIII., 4%. This result appears anomalous. Metrically the verses of Tennyson are superior to the verses of Browning, both as regards the alternation of centroids and the scarcity of sectional pauses. The explanation, probably, is that the subjects W. C. and S. are more irregular in their pauses than J. M. T. and C. O.

4. *Results.*—The range of duration for the pauses of speech was approximately 1.35^s (from 0.10^s to 1.45^s). This should hold for ordinary pauses.

Of the greater extreme deviations of pauses from the length of the average pause, approximately 88% are extremes of excess, 12% extremes of deficiency, and 0% equal extremes.

The duration of an average pause in speech is approximately 0.44^s.

As between prose and poetry, the difference in the average duration is inconsiderable. This was *apriori* to be expected. The amount of rest required in both cases is the same. The pause is a physiological and psychological resting period.

The range in the duration of the average pauses of a given number of reciters will perhaps be about $\frac{1}{2}^s$ (from 0.21^s to 0.72^s).

The elimination of punctuation marks slightly increases the number, and decreases the average duration of pauses.

The duration of the average pause of the speech of the same person on different occasions is never exactly the same. It will vary ordinarily from about 0.01^s (smallest difference) to 0.23^s (largest difference).

The longest and shortest averages are found, apparently, in colloquial prose and routine scansion, respectively.

The average irregularity in the length of pauses in speech is equal to approximately 0.15^s, or about 30% of the average duration.

In respect to units of time, the amount of irregularity in the pauses of different speakers will vary within the limits of about 0.01^s and 0.28^s (a range of 0.27^s).

In respect to the length of the average, the irregularity of the pauses of a given number of persons will be limited to a range of about 43% (from 4% to 47%).

The coordination of the lengths of pauses is slightly more exact in poetry than in prose.

The elimination of punctuation marks, apparently, does not tend to increase the inequality of the coordinaton.

The regularity is most exact in routine scansion.

With a provisional criterion of 10 per cent. of permissible inequality, approximately 8 per cent. of the records of pauses of speech are rhythmically coordinated in length.

5. *Distribution of the pauses in poetry.—Explanation.*—A sectional pause is a vacant interval that occurs within a verse; a terminal pause, one that occurs at the end of a verse—that is, between two verses of poetry. The former is an intra-verse, the latter an inter-verse, pause.

The term sectional pause has been applied to the pauses of such poetry as contains intra-verse punctuation marks. A given verse may frequently contain one or more, or none of these. By some writers the term is limited to what is technically designated the "cæsura," which is defined as a mid-verse pause occurring within a foot. In the sense in which the word is here employed it denotes any pause, given in the records, occurring within a verse.

The phrase "end-stopped line" is employed to denote a verse which is separated from the following verse by a punctuation mark. A "run-on line" is one that is not thus separated. In the run-on verses the punctuation mark is generally shifted to the first part of the following verse, where the pause should logically occur. The records present a variety of both kinds of verses.

The term terminal pause, in preference to the uncouth term end-line pause, will be employed to signify any pause that separates verses, whether run-on or end-stopped.

We shall tabulate the measurements under these two heads.

Measurements of —

(a) *Sectional pauses.*

TABLE XXVII.

(*Poetry.*)

Subject.	*d*	*a*	*p*	$\frac{p}{a}$	n_1	n_2
J. W. R.	3.40	0.48	0.16	0.33	7	7
E. W. S.	0.16	0.16			1	0
A. D. B.	4.37	0.33	0.10	0.30	13	4
W. W.	12.61	0.54	0.15	0.27	23	14
A. R. P.	0.20	0.20			1	0
H. Ö.	0.77	0.25	0.03	0.12	3	0
O. S.					0	2
Av.	3.58	0.32	0.11	0.25	6.8	3.8

TABLE XXVIII.

(*Poetry read as prose.*)

Subject.	*d*	*a*	*p*	$\frac{p}{a}$	n_1	n_2
J. M. T.	3.90	0.35	0.12	0.34	11	8
C. O.	4.85	0.37	0.11	0.29	13	8
W. C.	1.57	0.31	0.11	0.35	5	2
S.	1.92	0.38	0.11	0.28	5	2
Av.	3.06	0.35	0.11	0.31	8.5	5

TABLE XXIX.

(*Poetry read as poetry.*)

Subject.	*d*	*a*	*p*	$\frac{p}{a}$	n_1	n_2
J. M. T.	6.22	0.37	0.15	0.40	11	8
C. O.	5.16	0.39	0.12	0.30	13	8
W. C.	2.22	0.44	0.15	0.34	5	2
S.	3.80	0.47	0.13	0.27	8	2
Av.	4.35	0.41	0 13	0.32	9.2	5

TABLE XXX.

(*Prose read as poetry.*)

Subject.	*d*	*a*	*p*	$\frac{p}{a}$	n_1	n_2
S. I.	1.27	0.25	0.03	0.12	5	3
O. E. S.	1.81	0.36	0.05	0.13	5	3
Av.	1.54	0.30	0.04	0.12	5	3

TABLE XXXI.

(*Poetry read as prose without punctuation marks.*)

Subject.	d	a	p	$\frac{p}{a}$	n
J. M. T.	5.45	0.38	0.09	0.23	14
G. F. A.	6.15	0.47	0.15	0.31	13
B. S. G.	4.00	0.50	1.19	0.38	8
Av.	5.20	0.45	0.14	0.30	11.6

TABLE XXXII.

(*Summaries of sectional pauses of poetry.*)

Table.	d	a	p	$\frac{p}{a}$	n_1	n_2
XXVII.	3.58	0.32	0.11	0.25	6.8	3.8
XXVIII.	3.06	0.35	0.11	0.31	8.5	5.
XXIX.	4.35	0.41	0.13	0.32	9.2	5.
XXX.	1.54	0.30	0.04	0.12	5.	3.
Av.	3.13	0.34	0.09	0.25	7.37	4.2

Unit of measurement, 1^s. d, total duration; a, average duration; p, immediate probable error; $\frac{p}{a}$, relative immediate probable error; n_1, number of pauses; n_2, number of punctuation marks.

(b) *Terminal pauses.*

TABLE XXXIII.

(*Poetry.*)

Subject.	d	a	p	$\frac{p}{a}$	n_1	n_2
J. W. R.	16.60	0.77	0.25	0.32	21	20
E. W. S.	12.00	0.44	0.11	0.25	27	22
A. D. B.	6.02	0.75	0.24	0.32	8	5
W. W.	8.64	0.61	0.13	0.21	14	13
H. Ö.	3.87	0.55	0.10	0.18	7	5
O. S.	2.93	0.42	0.09	0.21	7	5
A. R. P.	1.62	0.54	0.16	0.29	3	2
Av.	7.38	0.58	0.15	0.25	12.4	10.2

TABLE XXXIV.

(*Poetry read as prose.*)

Subject.	d	a	p	$\frac{p}{a}$	n_1	n_2
J. M. T.	1.62	0.54	0.06	0.11	3	3
C. O.	2.12	0.70	0.05	0.07	3	3
W. C.	6.37	1.06	0.10	0.09	6	6
S.	5.52	0.92	0.16	0.17	6	6
Av.	3.90	0.80	0.09	0.11	4.5	4.5

TABLE XXXV.

(*Poetry read as poetry.*)

Subject.	d	a	p	$\frac{p}{a}$	n_1	n_2
J. M. T.	2.31	0.77	0.05	0.06	3	3
C. O.	2.25	0.56	0.15	0.26	4	3
W. C.	6.02	0.86	0.20	0.23	7	6
S.	4.95	0.82	0.12	0.14	6	6
Av.	3.88	0.75	0.13	0.17	5	4.5

TABLE XXXVI.

(*Prose read as poetry.*)

Subject.	d	a	p	$\frac{p}{a}$	n_1	n_2
S. I.	1.95	0.65	0.07	0.10	3	3
O. E. S.	1.75	0.58	0.06	0.10	3	3
Av.	1.85	0.61	0.06	0.10	3	3

TABLE XXXVII.

(*Poetry read as prose without punctuation marks.*)

Subject.	d	a	p	$\frac{p}{a}$	n_1	n_2
J. M. T.	0.62	0.20	0.03	0.15	3	3
G. F. A.	1.02	0.51	0.24	0.47	2	3
B. S. G.	2.62	0.37	0.10	0.27	7	6
Av.	1.42	0.36	0.12	0.29	4	4

TABLE XXXVIII.

(*Summaries of terminal pauses of poetry.*)

Table.	d	a	p	$\frac{p}{a}$	n_1	n_2
XXXIII.	7.38	0.58	0.15	0.25	12.4	10.2
XXXIV.	3.90	0.80	0.09	0.11	4.5	4.5
XXXV.	3.88	0.75	0.13	0.17	5	4.5
XXXVI.	1.85	0.61	0.06	0.10	3	3
Av.	4.25	0.68	0.10	0.15	6.22	5.55

Unity of measurement, 1^s. d, total duration; a, average duration; p, immediate probable error; $\frac{p}{a}$, relative immediate probable error; n_1, number of pauses; n_2, number of punctuation marks.

Comparison of tables.—The study is based on 129 sectional pauses and 73 sectional punctuation marks; and on 122 terminal pauses and 114 terminal punctuation marks. This is exclusive of Tables XXXI. and XXXVII., poetry read as prose without punctuation marks.

Numerical relation of pauses to punctuation marks.—It is a trite obser-

vation, enforced by an examination of the records, that punctuation marks and pauses do not invariably coincide, and that pauses frequently occur irrespective of any visible symbol of punctuation. Furthermore, the tables warrant the assertion that the number of pauses, both sectional and terminal, exceeds the number of punctuation marks. That is to say, while, as the records show, the number of punctuation marks exceeds the number of correlative punctuation-mark pauses, the number of non-punctuation-mark pauses is so large that, for poetry in general, the pauses outnumber, quite considerably, the marks.

The proportion of the number of sectional pauses to sectional punctuation marks is as 5.37 : 4.2 ; of terminal pauses to terminal punctuation marks, as 6.22 : 5.55. In other words, the pauses occurring within the verses are 1.75 times more frequent than the corresponding punctuation marks; and those following, 1.12 times more frequent than the corresponding punctuation marks.

The highest number of sectional pauses occurring in any record is 23, W. W., Table XXVII., where there are fourteen punctuation marks for eighteen verses of Byron's Childe Harold. In several records the sectional pause, as well as punctuation mark, is entirely lacking.

The proportion of the number of terminal punctuation marks to sectional punctuation marks is as 5.55 : 4.2, or as 1.33 : 1.

While the punctuation mark is thus, for a given variety of verses, generally largely of inter-verse occurrence, in certain kinds of verse structure it is exclusively a matter of inter-verse occurrence ; yet in other cases the sectional punctuation marks are predominant. (As instances, see W. W. and C. O. records for the latter, and E. W. S., A. R. P. and H. Ö. records for the former).

The number of pauses occurring within the verse is proportioned to the number occurring at the end of the verse as 7.37 : 6.22, or as 1.18 : 1.

The proportion between the number of sectional and terminal pauses is thus smaller than the proportion between the number of sectional and terminal punctuation marks. Whereas the number of sectional punctuation marks is smaller than the number of terminal punctuation marks, the number of sectional pauses is greater than the number of terminal pauses. These facts may be interpreted as signifying that a certain artificiality obtains in the distribution of the punctuation marks in verse. The tendency is to unwarrantably eliminate the marks from the interior of the line. In reading, the deficiency of punctuation marks is restored by introducing extra sectional pauses. The tendency of the former should be to preserve the rhythm and obscure the thought, of the latter, to disturb the rhythm and elucidate the thought. Both principles find application in the records cited above.

The number of sectional pauses in Table XXVIII. is smaller than in the corresponding table (XXIX.), where the rendering was purposely poetical. This, possibly, explains why the latter does not sound more distinctly poetical than the former. Where the selections were read without punctuation marks, the number of sectional pauses, relatively to the number of punctuation marks in the corresponding records of Tables XXVIII. and XXIX., was increased. The number of terminal pauses, however, remains, relatively to the number of punctuation marks, the same as in the prose and poetry rendering of the same passage.

Relative total duration.—The average aggregate duration, Table XXXII., of the sectional pauses sustains the proportion to the terminal pauses (Table XXXVIII.) of 3.13^s: 4.25^s, or as 1:1.35. In respect of number of pauses the proportion is reversed—namely as 1.18:1. That is, while the number of sectional pauses is approximately 20% higher than the number of terminal pauses, the duration of the sectional pauses is approximately 33% less than the duration of the terminal pauses. The difference in duration is thus larger than the difference in number.

The most notable exception is afforded by Tables XXXI. and XXVII., where the proportion of the duration of the sectional pauses to the terminal pauses is as 3.66:1. The selection from Bryon, and the passages from Browning and Tennyson, also differ in this regard, the former (Browning) having the greater mass of silence within, the other (Tennyson) without, the verses. This suggests important considerations with reference to the metrical quality of the verses of the two types of poetry. The relatively greater number and aggregate duration of the sectional pauses tends to jeopardize the unity of the verse, and to cause to coalesce rather than segregate as distinct wholes, the successive verses. When the bulk of silence is thrown between, instead of within, the verses, they must needs stand forth in consciousness as unified groups of presentation complexes.

Relative average duration.—The duration of the average sectional pause is proportioned to that of the terminal as 0.34^s : 0.68^s, or as 1 : 2. In other words, the average terminal pause of a series of structurally different verses was precisely twice as long as the average sectional pause.

As has been suggested, this subordination of length of the sectional to the terminal pause, is probably the most potent principle of the segregation of verses into distinct groups.

The records in which the verse intervals most pronouncedly appear as distinct wholes are those in which the terminal pauses are predominant in the number of occurrences and in the quantity of duration (cf. E. W. S.,

O. S., A. R. P., H. Ö., and E. H. T. records). In the records in which this principle of subordination is violated, the unity of the verse interval is perceptibly disturbed (cf. W. W. record). When the principle of subordinating the sectional to the terminal pauses is reversed, the verse loses its character as a distinct unitary group. This is instanced in all the records where the punctuation marks were left out.

The observance of this principle largely gives the character of poetry to the passages that were read as prose. These passages with doubtful rhythm, sound more like poetry than prose. In fact, from this point of view, they are rather more poetical than the corresponding readings of the selections as poetry. Examination of the two series of records shows the following: the average sectional pause is 0.06^s shorter in the table (XXVIII.) for poetry read as prose than in the table (XXIX.) for poetry read as poetry; the terminal pause, 0.05^s longer. Naturally, we would expect the sectional pause to be shorter in the distinctly poetical renderings.

The observance of the rule of subordinating the length of the sectional to terminal pauses in the prose rendering of the set of test record, suggests that the structural (organic) and the logical pause arrangement in the verses coincide. Where the selection was printed as prose, no suggestions from the verse arrangement of the words could tend to make the terminal pauses longer. They were prolonged, evidently, because the subject felt that at those points in the sequence of words a long pause should naturally—because representing to the hearer a large stop or transition in the thought—be made. To preserve the integrity of the verse, the larger transitions in thought should be so arranged as to coincide with the terminal pauses. In the records of W. W. and J. M. T. this rule is not observed. The result is that the verses are split up and lose their unity, to retain which they would have to be scanned in such a fashion as to involve a sacrifice of thought. As between the two, the average reader prefers sacrificing rhythmical perfection to impairing the distinct articulation of the thought.

The introduction of the "cæsura" illustrates the same principle. The difference between the length of the cæsuras and the terminal pauses in the records is less than the difference between the other sectional pauses and the terminal pauses. The tendency of the cæsura is to split the verse up into two unities. The cæsural and the terminal punctuation marks should both coincide with naturally large thought transitions, to preserve the unity of the verse interval.

The subordination of pauses in verse finds its parallel in prose. The pauses between sentences are longer than those contained within sentences. The average for all kinds of pauses in prose (Table XXI.) is 0.56^s. The

average for the pauses at the end of sentences is 1.21^s. (The records afford only seven instances.) The relation is as 1:2.16.

The difference between the two is, that the terminal pauses of poetry are relatively equidistant. In prose the sentence may be of a great variety of lengths: hence the terminal pause in prose is not nearly so periodically recurrent as in poetry. This constitutes the essential difference, which is so easily heard, between the pauses of prose and poetry.

All the records are in complete agreement with the principle of pause subordination, except two of the records of Table XXXI., prose without punctuation marks, where the average of the sectional pauses is 0.45^s. The pauses of these records, according to the judgment of the ear, are placed with hesitancy and diffidence.

Regularity.—The absolute regularity of the two series differs only by 0.01^s. The relative inequality of the terminal pauses is 15%; of the sectional 25%. The terminal pauses are one and two-thirds times as regular as the sectional.

The regularity is higher in the prose reading of the test than in the corresponding reading of the passages as poetry.

The lowest inequality for any set of records of sectional pauses is 12%, Table XXX., prose read as poetry, the highest, 32%, Table XXIX., poetry read as poetry; a range of 20%.

The lowest inequality for the terminal pauses is 10%, Table XXXVI., prose read as poetry; the highest 29%, Table XXXVII., poetry read as prose without punctuation marks; a range of 19%.

The lowest degree of irregularity for any record of sectional pauses is 12%, records of H. Ö. and S. I; the highest, 40%, J. M. T.; a range of 28%.

For terminal pauses the lowest is 7%, C. O.; the largest 47%, G. F. A., the record without the punctuation marks; a range of 40%.

The range for the individual records of sectional pauses is 8% larger than the range for the sets: and, of the terminal pauses, 21% larger.

Results—

The number of pauses of all kinds in recited poetry is generally greater than the number of punctuation marks.

The number of punctuation-mark pauses is frequently if not generally slightly less than the number of punctuation marks, in recited poetry.

The proportion of the number of pauses to punctuation marks varies according as they are sectional or terminal. In respect to the sectional pauses, there were approximately 7 for every 4 punctuation marks; and for the terminal pauses, approximately 10 for every 9 punctuation marks.

The terminal punctuation marks for a varied collection of verses seem to be about $1\frac{1}{3}$ times the frequency of the sectional punctuation marks.

The frequency of sectional pauses, for a variety of verses, is approximately $1\frac{1}{5}$ times that of terminal pauses. Most punctuation marks, for a variety of verses, seem to be terminal; most pauses, sectional.

The combined length of the terminal pauses is approximately $1\frac{1}{3}$ times that of the sectional pauses. In some verses, however, the aggregate length of the latter may be approximately $3\frac{2}{3}$ times that of the former.

The average terminal pause in a variety of verses is about twice as long as the average sectional pause.

The subordination of sectional to terminal pauses helps largely to unify the verses into distinct wholes.

The terminal pauses of poetry occur, with a high degree of regularity, at relatively equidistant points; those of prose, at indefinite distances. The length of sentences in prose admits of nominal coordination; of verses in poetry, of a high degree of coordination. The latter may, and often do, become rhythmical; the former never or rarely do.

Removing punctuation marks reverses the rule of subordination: the terminal pauses become subordinate to the sectional. The latter are increased in both length and frequency when compared with records of the same selections with the marks; the former remain relatively the same. This reversal tends to destroy the unity of verse intervals.

The irregularity of the terminal pauses of poetry is about 15% of the average duration; of the sectional, 25%. The former are about $1\frac{2}{3}$ times more regular than the latter. This suggests that sectional pauses are a more important disturbing factor of the rhythm of verses than terminal.

The irregularity of the sectional pauses for different persons ranges from about 12% to 40% (28%); of the terminal pauses, from about 7% to 47% (40%) of the average.

Accepting 10% as the limit of inequality for rhythmical units, none of the records of sectional pauses, and only 25% of terminal pauses, are rhythmically coordinated in length.

6. *The time value of punctuation marks.*

Two divergent views have obtained: Complete lawlessness reigns among the different punctuation marks with respect to their relative time value; and, they may be arranged in a scale of values, whether or not the temporal and logical values correspond.

The tables are arranged in two groups: punctuation-mark pauses, and non-punctuation-mark pauses. The former correspond, in the records,

with the symbols which occur between the same words in print. These symbols were directly perceived, perhaps vaguely, as the selections were read: and were pictured perhaps, consciously or unconsciously, to the imagination, when the words were recited without looking at the printed page.

The term non-punctuation-mark pauses, however uncouth, indicates pauses in the records between the words of which there are no punctuation marks upon the printed page. These pauses are of two kinds: those purely accidental, and those conducive to emphasis. The latter may be emotional or logical. These add clearness and emphasis to the thought. This is not true of the fortuitous; they tend rather to obcure. In the table the division into logical and emotional is disregarded. A punctuation mark may frequently be inserted where they occur. The division into accidental and emphasis has been made by the judgment of the experimenter's ear, on the basis of the context or the mode of delivery. Terminal non-punctuation-mark pauses have generally been regarded as emphasis pauses.

Measurements of punctuation-mark pauses.

TABLE XXXIX.

(;)

Subject.	*d*	*a*	*p*	$\frac{p}{a}$	*n*
J. W. R.	1.79	0.89	0.03	0.03	2
E. W. S.	2.52	0.63	0.11	0.17	4
A. D. B.	2.33	1.16	0.00	0.00	2
A. R. P.	1.20	0.60	0.21	0.35	2
G. A. A.	0.57	0.57			1
W. L. P.	1.16	1.16			1
C. O. S.	2.27	0.75	0.12	0.16	3
W. C.	2.27	1.13	0.05	0.04	2
S.	1.80	0.90	0.16	0.17	3
S. I.	1.42	0.71	0.00	0.00	2
O. E. S.	1.28	0.64	0.00	0.00	2
W. C. [illegible]	2.47	1.23	0.00	0.00	2
S. A.	1.77	0.88	0.08	0.09	2
Total	22.85				28
Av.		0.86	0.07	0.09	

TABLE XL.

(.)

Subject.	*d*	*a*	*p*	$\frac{p}{a}$	*n*
W. L. P.	1.45	1.45			1
G. A. A.	3.72	0.93	0.10	0.10	4
C. O. S.	0.90	0.90			1
O. S.	0.62	0.62			1
H. Ö.	0.82	0.82			1
C. O.	0.62	0.62			1
J. M. T.	0.87	0.87			1
C. $O._A$	0.64	0.64			1
Total	9.64				11
Av.		0.85	0.10	0.10	

TABLE XLI.

(,-)

Subject.	*d*	*a*	*p*	$\frac{p}{a}$	*n*
W. W.	1.66	0.83	0.00	0.00	2
W. L. P.	1.12	1.12			1
J. M. T.	1.45	0.72	0.03	0.04	2
C. O.	1.06	0.53	0.00	0.00	2
Total	5.29				7
Av.		0.80			

TABLE XLII.

(!)

Subject.	*d*	*a*	*p*	$\frac{p}{a}$	*n*
J. W. R.	1.10	1.10			1
W. W.	5.37	0.76	0.05	0.06	7
W. L. P.	1.87	0.93	0.58	0.62	2
J. M. T.	1.64	0.54	0.19	0.35	3
C. $O._B$	2.02	0.67	0.03	0.04	3
C. $O._A$	1.85	0.61	0.14	0.22	3
Total	13.85				19
Av.		0.77	0.19	0.26	

Table XLIII.

(,)

Subject.	d	a	p	$\frac{p}{a}$	n
J. W. R.	10.29	0.68	0.19	0.28	15
E. W. S.	5.43	0.40	0.02	0.04	11
A. D. B.	4.64	0.66	0.16	0.24	7
W. W.	7.56	0.47	0.12	0.25	16
H. Ö.	1.47	0.49	0.05	0.10	3
O. S.	1.81	0.45	0.09	0.20	4
G. A. A.	6.20	0.48	0.09	0.18	13
W. L. P.	3.77	0.75	0.21	0.28	5
C. O. S.	1.70	0.42	0.13	0.30	4
J. M. T.	2.06	0.68	0.06	0.08	3
C. O.	1.12	0.28	0.07	0.25	4
W. C.	4.75	0.79	0.13	0.16	6
S.	4.50	0.75	0.13	0.17	6
S. I.	0.87	0.29	0.03	0.10	3
O. E. S.	1.12	0.39	0.02	0.05	3
C. $O._A$	2.70	0.45	0.09	0.20	6
W. $C._A$	4.67	0.77	0.21	0.27	6
$S._A$	4.77	0.79	0.21	0.26	6
Total	69.43				121
Av.		0.56	0.11	0.18	

Table XLIV.

(—)

Subject.	d	a	p	$\frac{p}{a}$	n
E. W. S.	1.75	0.43	0.09	0.20	4
J. M. T.	0.57	0.57			1
S. I.	0.52	0.52			1
O. E. S.	0.46	0.46			1
C. O.	0.66	0.66			1
Total	3.96				8
Av.		0.53	0.09	0.20	

Table XLV.

(:)

Subject.	d	a	p	$\frac{p}{a}$	n
E. W. S.	0.60	0.30	0.06	0.20	2
C. O. S.	0.57	0.28	0.15	0.53	2
H. Ö.	0.37	0.37			1
W. L. P.	1.10	1.10			1
C. $O._B$	0.45	0.45			1
J. M. T.	0.62	0.62			1
C. O.	0.56	0.56			1
Total	4.27				9
Av.		0.52	0.10	0.36	

TABLE XLVI.

(Summaries for punctuation-mark pauses)

Table.	d	a	p	$\frac{p}{a}$	n
XXXIX. (;)	22.85	0.86	0.07	0.09	27
XL. (.)	9.64	0.85	0.10	0.10	11
XLI. (,-)	5.29	0.80			7
XLII. (!)	13.85	0.77	0.19	0.26	19
XLIII. (,)	69.43	0.56	0.11	0.18	121
XLIV. (—)	3.96	0.53	0.09	0.20	8
XLV. (:)	4.27	0.52	0.10	0.36	9
Total	129.29				202
Av.		0.70	0.11	0.19	

Unit of measurement, 1^s.

d, duration.

a, average duration.

p, immediate probable error.

$\frac{p}{a}$, relative immediate probable error.

n, number of punctuation marks.

Measurements of non-punctuation-mark pauses.

TABLE XLVII.

(Emphasis)

Subject.	d	a	p	$\frac{p}{a}$	n
J. W. R.	2.14	0.42	0.15	0.35	5
E. W. S.	1.11	0.27	0.07	0.25	4
A. D. B.	2.45	0.27	0.10	0.37	9
W. W.	4.07	0.50	0.17	0.34	8
A. R. P.	0.42	0.42			1
O. S.	0.34	0.34			1
H. Ö.	1.95	0.39	0.13	0.33	5
W. L. P.	2.68	0.44	0.15	0.34	6
G. A. A.	0.87	0.43	0.03	0 07	2
C. O. S.	2.02	0.25	0.04	0.16	8
J. M. T.	0.58	0.29	0.11	0.37	2
C. O.	0.95	0.23	0.05	0.21	4
W. C.	0.90	0.30	0.05	0.16	3
S.	1.42	0.47	0.16	0.34	3
S. I.	0.60	0.30	0.02	0.06	2
O. E. S.	0.43	0.43			1
Total	22.93				64
Av.		0.36	0.09	0.25	

TABLE XLVIII.

(*Accidental*)

Subject.	d	a	p	$\frac{p}{a}$	n
J. W. R.	0.47	0.23	0.06	0.26	2
E. W. S.	0.31	0.15	0.00	0.00	2
A. D. B.	0.93	0.31	0.04	0.12	3
W. W.	0.95	0.47	0.00	0 00	2
A. R. P.	0.20	0.20			1
O. S.	0.31	0.31			1
W. L. P.	0.54	0.27	0.00	0.00	2
C. O. S.	0.27	0.27			1
J. M. T.	0.91	0.91			1
C. O.	1.00	0.50	0.14	0.28	2
W. C.	0.32	0.32			1
S.	1.02	0.34	0.15	0.44	3
O. E. S.	0.25	0.25			1
Total	7.48				22
Av.		0.34	0.05	0.15	

TABLE XLIX.

(*Summaries of non-punctuation-mark pauses*)

Table.	d	a	p	$\frac{p}{a}$	n
XLVII. (Emphasis)	22.93	0.36	0.09	0.25	64
XLVIII. (Accidental)	7.48	0.34	0.05	0.15	22
Total	30.41				86
Av.		0.35	0.07	0.20	

Unit of measurement, 1^s. d, total duration.
a, average duration. p, immediate probable error.
$\frac{p}{a}$, relative immediate probable error. n, number of pauses.

Results.—Two hundred and eighty-eight pauses were measured, 202 punctuation-mark and 86 non-punctuation-mark. Unfortunately several of the punctuation-marks occurred so infrequently as to render some of the results entirely provisional. Far more measurements are needed.

In respect to the frequency of pauses occurring in speech, approximately 75 per cent. are punctuation-mark pauses. The balance are non-punctuation-mark pauses. In the records, 60% of the former, were commas, 13% semicolons, 9% exclamation-marks, 5% periods, 4% colons, 3% dashes and 3% comma-dashes (,-). The records of poetry, although more numerous than those of prose, contain: of semicolons

66%, periods 45%, comma-dashes 86%, exclamation-marks 87%, commas 72%, colons 62%, and dashes 75%. This may possibly indicate that the periods are more abundant in prose, and the exclamation-marks and comma-dashes in poetry.

About 75% of the non-punctuation-mark pauses are emphasis pauses. These tend to contribute strength and perspicuity to, and the accidental pauses to detract these from, the thought of the sentences. The accidental pauses, as was to be expected in rational, coherent speech, are very rare (about 7% of all pauses).

Apparently, over 40% of the pauses of speech are comma pauses. The relative frequency of the different punctuation marks differs somewhat for poetry and prose, and for different styles of writing.

The punctuation-mark pauses in the records occupy about 80% of the aggregate duration of all kinds of pauses. Of the former, commas occupy 53%; semicolons, 17%; exclamation-marks, 10%; periods, 7%; comma-dashes, 4%; colons, 3%; and dashes 3%. The time subordination corresponds with that of frequency, but the percentages do not exactly correspond.

The time value of the average punctuation-mark pause is about twice that of the non-punctuation-mark pause (former in records $= 0.70^s$; latter $= 0.35^s$).

The shortest average punctuation-mark pause (:) is seemingly longer than the longest average non-punctuation-mark pause. The difference was about 0.16^s. Only nine of the former were measured. It may be somewhat uncertain.

The longest average punctuation-mark pause is considerably longer than the shortest average punctuation-mark pause [about 0.34^s; from 0.52^s (:) to 0.86^s (;)].

The average accidental and emphasis pauses appear to be about equal in length (range, 0.02^s).

The duration of an average punctuation-mark pause of spoken poetry and prose is about 0.70^s.

Punctuation marks differ in respect to the time which they occupy in speech, both absolutely and relatively. Generally, no two consecutive punctuation marks, of the same or different kind, are exactly coordinated in length. The longest and shortest averages of the tables are as follows: semicolons, 1.23^s, and 0.57^s (range, 0.66^s); periods (Table XL.), 1.45^s and 0.62^s (range, 0.83^s); comma-dashes (Table XLI.), 1.12^s and 0.53^s (range, 0.59^s); exclamation-marks (Table XLII.), 1.10^s and 0.54^s (range, 0.56^s); commas (Table XLIII.), 0.79^s and 0.28^s (range, 0.51^s); dashes (Table XLIV.), 0.66^s and 0.43^s (range, 0.23^s); and colon pauses

1.10^s and 0.37^s (range, 0.73^s). The range for the three punctuation-mark pauses of greatest frequency follows the subsumption according to the average duration: it is greatest for the semicolon, and least for the comma, pauses. The difference in the ranges of these is 0.15^s.

The average lengths of different punctuation marks obey a rule of subordination, more or less indefinite in details. In the main, we can arrange them in three groups: (1) the semicolon and period; (2) the exclamation-point and comma-dash; and (3) the comma, dash and colon. The interrogation point presumably belongs to one of the first two. The approximate value of the first group is 0.86^s, of the second, 0.78^s, and the third, 0.53^s. No fixed line of demarkation exists between the groups. The difference between (1) and (2) (it is possible to combine them into one) appears less marked than between (2) and (3).

The regularity of punctuation and non-punctuation-mark pauses is about equal. The average amount of irregularity is approximately 0.10^s, or 19% of the average duration.

Apparently, commas are more irregular than semicolons. Exclamation-marks are doubtful, owing to extreme irregularity of W. L. P. The frequency of the other punctuation-marks is insufficient to justify any conclusions.

The range of irregularity for the punctuation-mark pauses of a given number of speakers is, in units of time, about 0.19^s (from 0.02^s to 0.21^s); and, as a fraction of the average, about 50% (from 3% to 53%). This is slightly larger than for non-punctuation mark pauses (0.15^s, or 38%).

Accepting tentatively the 10% criterion, 43% of the semicolons, 40% of the exclamation-marks, 28% of the commas, 15% of the emphasis pauses and 0% of the accidental pauses, are rhythmically coordinated in length. The figures for the comma and emphasis pauses are the most trustworthy.

The concept of the punctuation mark.—Upon the printed page it is a symbol, like any other character of type, meaningless apart from an interpreting mind. Through experience, it acquires a definite connotation just like any of the letters.

Every character upon the page has a twofold significance for speech: it represents a physiological and a psychological process. The latter is conditioned upon a presentation of sense; and is translated into a definite motor innervation as soon as it is apperceived. When the characters are letters, in isolation or in combination, the apperception of a letter or word expresses itself in a definite adjustment of the larynx, giving rise to a definite sound.

Similarly, punctuation marks are meaningless presentations until

apperceived. They acquire a definite meaning according to the way the contours differ. The significance of any cognate characters depends upon such distinguishing traits of contour. A . and a ;, just as an *s* and a *c*, are connected with distinct apperceptions.

As interpreted presentations (apperceptions), punctuation marks denote breaks or transitions in the continuity of the thought and differences in the quality or character of the pause. A period denotes the termination of a single wave (pulse or oscillation) of thought ; a semicolon, a ripple in the wave without being a break ; and the comma, a minor ripple. The two latter are a species of subordinate waves comprehended in the unity of the whole wave. There are several such species of thought-waves, *e. g.*, those of declaration, interrogation, exclamation. In the main, the different punctuation marks have as fixed a signification for thought as do the letters of the alphabet.

Finally, as motor resultants they denote physiological processes differing according as the marks and the character of the waves of thought differ. The physiological differences are threefold : (1) *As to modulation.* The pitch of the voice varies according to the character of the sign. This is a subject for special research. (2) *As to timbre.* This is specially noticeable in the bracket and parenthesis (or in the parenthetical commas), question-mark and exclamation-mark. This subject also merits special investigation. (3) *As to the pause.* This is perhaps the most obvious difference. Punctuation marks, physiologically considered, are pauses, the average length of which varies for the different marks, as has been shown above.

It is largely upon the basis of these three characteristics, that a listener can punctuate the speech of a lecturer, who is observing in his speech, evidently unconsciously, the laws of the physiology of the punctuation mark.

Hence a given punctuation mark is on the one hand a visible symbol, directly perceived by the eye, or represented to the imagination—however vaguely—which signifies to the reader a turn or break in the movement of thought ; and, on the other, an auditory image which, as a moment in the physiological process of speech, is distinguished by changes in duration of pause, in pitch and quality of sound. In these respects it is a mental item, representing quantitative and qualitative peculiarities.

F. Centroid intervals.

By a centroid interval is understood the interim, spatial upon the page to the eye, temporal in speech to the ear, which stretches between two successive centroids, *i. e.*, from centroid *a*, to centroid *b*, from cen-

troid *b* to centroid *c*, and so on. Hence every centroid interval includes one centroid.

According to Brücke the centroid interval lies between two "Arsengipfeln"; according to Minor, it begins with the syllable standing in arsis; and according to Meyer,[1] when the beginning of the arsis coincides with the beginning of a syllable, and the end of the thesis with the end of a syllable, the limits of the foot and syllable coincide. Meumann[2] and Bolton[3] find experimentally that accented sounds occupy the first place in the interval. Our measurements extend as nearly as possible between two successive centroids.

In music the centroid interval is popularly designated a bar; in poetry, a foot or a measure; and in prose it has no distinctive name.

The centroids of speech and music are, as centroids, one and the same. The intervals between them are also as intervals, identical. All centroids can be subsumed under the laws of centroid composition; all the intervals, under those of centroid-interval composition.

Hence the term centroid interval is applicable to all sorts of human utterance—poetry, prose and music. No justification exists for drawing hard and fast lines between either the centroids or centroid intervals of these, although slight differences may, and do, obtain between the *regularity* of the intervals and the *relation* of the elements in the centroids of poetry, prose and music.

The composition of centroid intervals.—Only two kinds of materials can enter into their composition—sounds and silences, or syllables and pauses. A centroid interval may be composed purely of a sound-plenum, though not of a pause-plenum. It must always contain a certain quantum of sound.

Centroid intervals consisting of sounds, or *syllables*, only may be called *sound-centroid intervals;* those of sounds and silences, or *syllables* and *pauses*, *composite-centroid intervals.*

Sound-centroid intervals may be divided into the following varieties: (1) 1-*syllable* intervals. Here the centroid syllables are directly contiguous; no unemphatic syllable or pause intervenes between them. Hence the interval consists simply of one centroidal syllable. The existence of such intervals has been both denied[4] and affirmed.[5] The

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[1] Meyer, *Beiträge zur deutschen Metrik*, Neuere Sprachen, 1898 VI 136–138.

[2] Meumann, *Untersuchungen zur Psychologie und Aesthetik des Rhythmus*, Philos. Stud., 1894 X 303.

[3] Bolton, *Rhythm*, Am. Jour. Psych., 1893 VI 222.

[4] Abbott and Seeley, English Lessons for English People, 154, Boston 1880.

[5] Mayor, Chapters on English Metre, III, London 1886.

records furnish numerous instances of this type. (2) *2-syllable*, containing an intervening syllable, or one centroidal and one non-centroidal syllable. (3) *3-syllable*, composed of one centroidal and two non-centroidal syllables. (4) *4-syllable*, consisting of one centroidal and three non-centroidal syllables. (5) *5-syllable* centroid intervals, consisting of one centroidal and four non-centroidal syllables. These are exceedingly rare; the number in the records is too small for purposes of study. GURNEY[1] holds that no centroid interval can be composed of more than one centroidal and four non-centroidal syllables; and MAYOR,[2] that even three consecutive syllables without a metrical accent is impossible.

The *composite-centroid* intervals may be divided as follows: (1) *1-pause-1-syllable* intervals, consisting of one pause in addition to the centroidal syllable. (2) *1-pause-2-syllable*, consisting of one pause, one centroidal and one non-centroidal syllable, the pause preceding or following the unemphatic syllable. (3) *1-pause-3-syllable*, composed of one pause, one centroidal and two non-centroidal syllables. (4) *1-pause-4-syllable*, composed of one pause, one centroidal and three non-centroidal syllables. (5) *1-pause-5-syllable*, containing one pause, one centroidal and four non-centroidal syllables.

The records furnish instances, too scant for purposes of study, of other modes of composition, such as 2-pause-2-syllable, and 2-pause-1-syllable centroid intervals.

The lengths of each variety of the two groups of centroid intervals were measured.

Results.—The study is based on 69 1-syllable, 238 2-syllable, 47 3-syllable and 12 4-syllable intervals.

The average duration of the sound-centroid intervals of speech is about 0.51^s. This is about the same as Martius' period for subjective rhythmisation (0.50^s). The average for the 1-syllable intervals was 0.32^s; for the 2-syllable, 0.44^s; for the 3-syllable, 0.62^s; for the 4-syllable, 0.69^s.

Any 1-syllable interval occurring in speech may be presumed to fall within the limits of about 0.14^s and 0.56^s (range, 0.42^s); any 2-syllable, within 0.18^s and 0.79^s (range, 0.61^s); and any 3-syllable, within 0.37^s and 0.97^s (range, 0.60^s). (For the 4-syllable interval the measurements are insufficient). Any one of the different varieties may be presumed to fall within 0.14^s and 0.97^s (range, 0.83^s).

The range for the averages of different records is about as follows: For 1-syllable intervals, 0.18^s (from 0.25^s to 0.43^s); for 2-syllable, 0.17^s (from 0.36^s to 0.53^s); for 3-syllable, 0.40^s (from 0.49^s to 0.89^s); and

[1] GURNEY, The Power of Sound, 433, London 1880.

[2] MAYOR, Chapters on English Metre, III, London 1886.

Measurements of sound-centroid intervals.

TABLE L.

1-syllable sound-centroid intervals.

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. M. T.	0.28	0.05	0.17	+0.45 −0.14	+	14
C. O.$_B$	0.25	0.05	0.20	+0.43 −0.16	+	14
W. W.	0.31	0.05	0.16	+0.47 −0.18	+	17
E. W. S.	0.40	0.03	0.07		0	2
A. D. B.$_A$	0.27					2
G. A. A.	0.30	0.05	0.18	+0.47 −0.22	+	5
W. L. P.	0.29	0.07	0.24	+0.41 −0.22	+	3
C. O. S.	0.36	0.08	0.22	+0.50 −0.20	−	5
S. I.	0.38	0.05	0.13	+0.47 −0.32	+	3
O. E. S.	0.43	0.05	0.11	+0.56 −0.34	+	4
Av.	0.32	0.05	0.16			

TABLE LI.

2-syllable sound-centroid intervals.

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. M. T.	0.46	0.08	0.17	+0.75 −0.25	+	17
C. O.	0.36	0.06	0.16	+0.54 −0.18	0	14
S. I.	0.43	0.04	0.09	+0.52 −0.35	+	6
O. E. S.	0.48	0.09	0.18	+0.65 −0.21	−	8
W. W.	0.42	0.07	0.16	+0.66 −0.20	+	42
A. D. B.$_A$	0.53	0.10	0.18	+0.79 −0.31	+	6
E. W. S.	0.44					60
G. A. A.	0.41					33
W. L. P.	0.41					26
C. O. S.	0.52					26
Av.	0.44	0.07	0.15			

TABLE LII.

3-syllable sound-centroid intervals.

Subject.	a	p	$\frac{p}{a}$	e	c	n
W. W.	0.72	0.08	0.11	+0.97 —0.58	+	5
E. W. S.	0.49	0.04	0.08	+0.56 —0.37	—	12
A. D. $B._A$	0.56	0.14	0.25	+0.79 —0.37	+	3
G. A. A.	0.51	0.05	0.09	+0.62 —0.37	—	10
W. L. P.	0.60	0.08	0.13	+0.70 —0.41	—	4
C. O. S.	0.63	0.08	0.12	+0.75 —0.37	—	8
S. I.	0.61	0.06	0.09	+0.70 —0.50	—	3
O. E. S.	0.89					2
Av.	0.62	0.07	0.12			

TABLE LIII.

4-syllable sound-centroid intervals.

Subject.	a	p	$\frac{p}{a}$	e	c	n
J. M. T.	0.72					1
C. O.	0.60					1
G. A. A.	0.65	0.02	0.03	+0.70 —0.62	+	3
W. L. P.	0.68	0.08	0.11	+0.77 —0.54	—	3
C. O. S.	0.83					2
S. I.	0.62					1
O. E. S.	0.78					1
Av.	0.69	0.05	0.07			

Unit of measurement, 1^s.
a, average duration.
p, immediate probable error.
$\frac{p}{a}$, relative immediate probable error.
e, extreme intervals.
c, character of greater extreme.
n, number of intervals.

Measurements of composite-centroid intervals.

TABLE LIV.

1-pause-1-syllable composite-centroid intervals.

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
E. W. S.	0.74	0.11	0.14	+1.12 —0.52	+	11
A. D. B.	0.66					1
W. W.	0.98	0.14	0.14	+1.47 —0.72	+	11
J. M. T.	0.92	0.08	0.08	+1.04 —0.77	—	5
C. O.	0.64	0.16	0.25	+1.02 —0.43	+	6
W. L. P.	1.34	0.10	0.14	+1.75 —1.00	+	5
C. O. S.	0.75	0.32	0.42			2
S. I.	0.47					1
O. E. S.	0.65					1
Av.	0.68	0.16	0.19			

TABLE LV.

1-pause-2-syllable composite-centroid intervals.

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
E. W. S.	0.71	0.08	0.11	+0.89 —0.50	—	13
A. D. B.	1.07	0.28	0.26	+1.68 —0.70	+	7
W. W.	0.91	0.17	0.18	+1.66 —0.56	+	25
J. M. T.	0.79	0.15	0.19	+1.04 —0.47	—	7
C. O.	0.75	0.13	0.17	+1.06 —0.43	—	10
G. A. A.	0.95	0.17	0.17	+1.47 —0.62	+	14
W. L. P.	1.26	0.20	0.15	+1.66 —0.68	+	7
C. O. S.	0.78	0.21	0.26	+1.45 —0.47	+	11
S. I.	0.82					2
O. E. S.	0.90					3
Av.	0.89	0.17	0.18			

Table LVI.

1-pause-3-syllable composite-centroid intervals.

Subject.	a	p	$\frac{p}{a}$	e	c	n
E. W. S.	0.75					2
A. D. B.$_A$	0.82	0.25	0.30	+ 1.35 — 0.45	+	4
W. W.	1.31					1
G. A. A.	0.86	0.34	0.39	+ 1.47 — 0.50	+	3
W. L. P.	0.90	0.15	0.16	+ 1.29 — 0.70	+	5
C. O. S.	0.92	0.14	0.15	+ 1.25 — 0.77	+	4
S. I.	0.75					1
Av.	0.91	0.22	0.25			

Table LVII.

1-pause-4-syllable composite-centroid intervals.

Subject.	a	p	$\frac{p}{a}$	e	c	n
G. A. A.	1.05	0.15	0.14	+ 1.30 — 0.85	+	3
W. L. P.	1.93					1
S. I.	0.95	0.21	0.22		0	2
O. E. S.	1.04	0.13	0.12		0	2
Av.	1.24	0.16	0.16			

Table LVIII.

1-pause-5-syllable composite-centroid intervals.

Subject.	a	p	$\frac{p}{a}$	n
S. I.	1.02	0.21	0.20	2
O. E. S.	1.26	0.07	0.05	2
Av.	1.14	0.14	0.12	

Unit of measurement, 1^s.
a, average duration
p, immediate probable error.
$\frac{p}{a}$, relative immediate probable error.
e, extreme intervals.
c, character of greater extreme.
n, number of intervals.

for 4-syllable intervals, 0.23^s (from 0.60^s to 0.83^s). The range appears to be highest for the average 3-syllable intervals.

The length of the average sound-centroid interval is invariably proportioned to the number of syllables of which it is composed. This is true alike of the reading scansion of poetry and prose. Single intervals frequently depart from this rule.

The addition of a syllable to sound-centroid intervals increases their average duration about 30%. The addition to the 3-syllable interval appears to result in the smallest, and to the 2-syllable, in the largest increment. (Average 2-syllable, 37% longer than 1-syllable; 3-syllable, 40 % longer than 2-syllable; 4-syllable, 11% longer than 3-syllable.)

The difference between the lengths of the different intervals appears to be very slightly larger in poetry than in prose.

No sound-centroid intervals are of precisely the same length. The average inequality of the four kinds is about 0.06^s, or a trifle over 12% of the duration. Apparently it is very slightly smaller for the longer than the shorter intervals.

The extreme deviations of the 1-syllable (75% = +, 11% = −, 11% = 0) and 2-syllable (66% = +, 16% = −, 16% = 0) intervals are predominantly extremes of excess. For the others (for 3-syllable, 28% = +, 71% = −) the predominant extremes are liable to be those of deficiency. Of all the varieties, 58% were extremes of excess, 33% of deficiency, and 9% were equal.

The 2-syllable interval is the predominant sound-centroid interval of speech. This is true alike of prose and dactylic and trochaic verse. (For poetry and prose, 65% = 2-syllable; 18%, 1 syllable; 13%, 3-syllable; 3%, 4-syllable). The 5-syllable and 4-syllable are the most infrequent. They seem to be less frequent in poetry than in prose.

According to the criterion of 10% of irregularity, 11% of the 1-syllable, 16% of the 2-syllable and 43% of the 3-syllable sound-centroid intervals were rhythmically coordinated in length. The large percentage for the 3-syllable may indicate that centroid intervals of about 0.65^s are most easily rhythmized. For all varieties of sound-centroid intervals the figure is about 25%.

The study of the composite intervals is based on the measurement of 43 1-pause-1-syllable, 99 1-pause-2-syllable, 20 1-pause-3-syllable, 8 1-pause-4-syllable and 4 1-pause-5-syllable intervals.

The average duration of the five varieties of these intervals in speech was about 0.97^s.

It seems to be larger in proportion to the number of syllables of which the interval is composed. This rule meets with many variations in the individual records.

The difference between the averages of all the 1-pause-1-syllable and the 1-pause-2-syllable intervals was 0.21^s; between the latter and the 1-pause-3-syllable, 0.02^s.

Any single one of the five varieties occurring in ordinary speech may be presumed to fall within the limits of 0.43^s and 1.93^s (a range of 1.50^s). The range for the different varieties decreased progressively as the number of components increased. [Extremes for 1-pause-1-syllable$=1.75^s$ and 0.43^s (range, 1.32^s); for 1-pause 2-syllable$=1.66^s$ and 0.43^s (range, 1.23^s): for 1-pause-3-syllable$=1.47^s$ and 0.45^s (range, 1.02^s); for 1-pause-4-syllable $= 1.93^s$ and 0.85^s (range, 1.08^s).]

The longest and shortest averages were: for the 1-pause-1-syllable interval, 1.34^s and 0.47^s (range, 0.87^s); for the 1-pause-2-syllable, 1.26^s and 0.71^s (range, 0.55^s); for the 1-pause-3-syllable, 1.31^s and 0.75^s (range, 0.56^s). The different records of the same person, as well as those of different persons, gave a wide range of differences for the lengths of the different averages.

Approximately 80% of the extremes of the 1-pause-1-syllable, and 62% of the 1-pause-2-syllable intervals were extremes of excess. The balance were deficiency extremes. Of all the different varieties, about 70% were extremes of excess, 20% of deficiency and 10% of equality.

No intervals are of equal duration. The average inequality for the five different modes of composition was about 0.17^s, or 18% of the duration.

The most frequent mode of composition, both in prose and in poetry, was the 1-pause-2-syllable. It was about 8% more frequent in the records of poetry than in those of prose (52% of all modes).

The 1-pause-5-syllable, followed by the 1-pause-4-syllable, was the most infrequent mode of collocation. Both were less frequent in poetry than in prose. (In records of prose, the former = 4%, the latter 11%; in those of poetry both = 0%.)

On the basis of the 10% standard, 16% of the 1-pause-1-syllable, 6% of the 1-pause-2-syllable and 9% of all composite-centroid intervals are rhythmically coordinated.

Comparison of sound-centroid and composite-centroid intervals.—The average composite-centroid interval of speech occupies about 1.90 times more time than the sound-centroid interval.

The extremes of both are predominantly extremes of excess, the percentage for the composite being about 1.20 times higher than for the sound-centroid intervals.

The range for a given composite interval of speech is likely to be about 1.80 times larger than that for a given sound-centroid interval.

The latter is relatively one and a half times more regular than the former.

The difference in the length of the several modes of composition of the two kinds of intervals is more pronounced, and less variable, in the case of the sound-centroid intervals.

In both, the 2-syllable mode of composition (over 50% of all modes) occurs most frequently and the 5- and 4-syllable combination least frequently.

(*a*) *The distribution of the syllables of centroid intervals.—Modes of distribution.* In the 1-syllable interval, the centroid extends throughout its entire length. This interval is customarily called a spondee. The definition of this as "a foot consisting of two equally accented syllables," is based upon the misconception that a centroid interval consists of more than one centroid. When the components of a series of intervals are equally strong (are real centroids as the theory supposes) each centroid constitutes a so-called foot: there is no reason for including two centroids in the interval, rather than three or four, or all in a spondaic verse. The spondee of prosody is thus based upon a misconception and a certain arbitrariness. The spondee, to mean anything, should signify a 1-syllable centroid interval; and as signifying this, it may be a convenient term. It is arbitrary and unwarranted to include two centroid intervals in one interval.

The 1-syllable interval is limited to short sequences. In long ones the tendency is to so emphasize certain of the centroids as to make the others subordinate.

The 2-syllable interval admits of two situations: (1) a non-centroidal followed by a centroidal syllable (the iambus, ⏑ –). Here the centroid closes the interval, unlike the next; (2) a centroidal followed by a non-centroidal syllable (the trochee, – ⏑).

The components of the 3-syllable interval admit of this distribution: (1) two non-centroidal syllables followed by one centroidal (the anapest, ⏑ ⏑ –); (2) one centroidal followed by two non-centroidal syllables (the dactyl, – ⏑ ⏑); (3) one non-centroidal, one centroidal and one non-centroidal syllable (the amphibrach or amphiambus, ⏑ – ⏑).

In the 4-syllable interval this distribution is possible: (1) one centroidal followed by three non-centroidal syllables; (2) three non-cen-

troidal syllables followed by one centroidal. This interval appears to be generally the opening interval of a sequence. No names have been applied to these two modes: (3) one centroidal, two non-centroidal, followed by one centroidal syllable, (the *choriamb*, – ⏑ ⏑ –, in ancient prosody). This mode is spurious, if the centroidal syllables are genuine centroids.

Bearing on the conceptions and nomenclature of prosody.—Our treatment of the centroid and centroid interval suggests two general lines of criticism.

First, as long ago pointed out by SHELLEY,[1] the distinction between "measured" and "unmeasured" speech is unscientific. Both prose and poetry contain centroids and centroid intervals, following the same laws of composition and distribution. It is inadmissible to restrict the application of "measure" or "foot" to poetry alone. Centroid intervals pervade all uttered language.

Second, as pointed out by GURNEY, the terms "foot," "measure" or "bar" are inapt. "The arrangement of the foot is a mere matter of the eye. Nothing can prevent ictus from being ictus."[2] The fundamental concept of the centroid interval in speech is that of a unitary whole, juxtaposed to, and coalescent with, other similar wholes, requiring a portion of time for the voice to pass over its sounds. These intervals between force centers may be called centroid-intervals, bars, feet, or measures. But, unless interrupted by pauses, no divisions or "bars" exist between them. Nor are centroid-intervals to be conceived as a succession of bars of invariable length, nor as "feet" of similar length, nor as successive quanta of time conforming to an invariable "measure." Moreover, these terms, besides being customarily restricted to poetry, are applied with different meanings to classical and modern poetry.[3] "In English poetry – – – – – the names of feet denote groups of accented and non-accented syllables, without reference to quantity" (ABBOTT and SEELEY). On the whole, the term centroid interval seems preferable.

Third, a convenient nomenclature for the different modes of distribution of the centroidal and non-centroidal syllables of the intervals is desirable. The old terminology of prosody is perhaps associated with misconceptions, as already indicated with reference to the spondee and choriamb.

Fourth, there is no physical distinction between the several *types* of the different modes of distribution. There may be a mental, or felt,

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[1] SHELLEY, A Defense of Poetry, Works, VII 6, London 1880.

[2] GURNEY, The Power of Sound, 426, London 1880.

[3] ELLIS, The Quantitative Pronunciation of Latin, 5, London 1874.

difference between the iambus and trochee, and the anapest and dactyl; but physical measurements of them always extend from centroid to centroid, independently of type differences. The modes of distribution are, upon the whole, the same in prose and poetry. Prose, however, observes no systematic arrangement of the different kinds of intervals throughout the sentences in this respect. In poetry, the verses of a given stanza are supposed to consist of intervals of the same mode of distribution, or, if of different modes, to follow an orderly arrangement.

The succession or recurrence of centroid intervals.—As recurrences within sequences, they may be divided into two classes. In the *complex centroid intervals* are included all the intervals occurring between the first and the last centroid of a given record, irrespective of the composition of the intervals. It includes sound-centroid and composite-centroid intervals.

Simple-centroid intervals include all those occurring within expiration groups, or all the varieties of sound-centroid intervals. The latter were studied, not as successive members of rhythmical sequences, but as intervals, whether in isolation or succession, different in composition and time-value.

Each expiration group is an uninterrupted sequence of simple-centroid intervals. A pause destroys this continuity.

A comparison of these two intervals, the one including pauses, the other not, will determine the *differentia* of language rhythm, the function of the pause, etc.

Measurements of complex-centroid intervals.

TABLE LIX.

(Reading scansion, English poetry)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. W. R	0.74	0.29	0.38	1.81	+	115
				—0.33		
E. W. S.	0.52	0.10	0.19	+1.12	+	102
				—0.25		
A. D. B.$_A$	0.75	0.25	0.33	+1.68	+	24
				—0.27		
A. D. B.$_B$	0.72	0.16	0.22	+1.47	+	16
				—0.45		
W. W.	0.60	0.21	0.35	+1.66	+	101
				—0.18		
A. R. P.	0.51	0.15	0.29	+1.20	+	15
				—0.27		
Av.	0.64	0.19	0.29			

Table LX.

(*Reading scansion, Japanese poetry*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. K.	0.32	0.06	0.18	+0.52	+	13
				—0.16		
I. M.	0.39	0.16	0.42	+1.11	+	14
				—0.18		
K. H. K.	0.72	0.27	0.37	+1.54	+	15
				—0.31		
Av.	0.47	0.16	0.32			

Table LXI.

(*Sing-song and doggerel scansion*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
H. Ö.	0.65	0.16	0.24	+1.37	+	31
				—0.47		
O. S.	0.68	0.13	0.19	+1.31	+	28
				—0.47		
Av.	0.66	0.14	0.21			

Table LXII.

(*Routine scansion*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
E. H. T.$_A$	0.50	0.07	0.14	+0.70		15
				—0.31	+	
E. H. T.$_B$	0.48	0.02	0.04	+0.58		15
				—0.43	+	
Av.	0.49	0.04	0.09			

Table LXIII.

(*Summary of complex-centroid intervals of poetry*)

Table.	*a*	*p*	$\frac{p}{a}$
LIX.	0.64	0.19	0.29
LX.	0.47	0.16	0.32
LXI.	0.66	0.14	0.21
LXII.	0.49	0.04	0.09
Av.	0.55	0.13	0.22

TABLE LXIV.

(*Prose*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
G. F. A.	0.55	0.19	0.32	+ 1.47 — 0.22	+	71
W. L. P.	0.70	0.28	0.40	+ 1.03 — 0.23	+	35
C. O. S.	0.63	0.17	0.26	+ 1.45 — 0.20	+	61
Av.	0.62	0.21	0.32			

TABLE LXV.

(*Poetry read as prose*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. M. T.	0.46	0.13	0.27	+ 0.85 — 0.14	+	48
C. O.	0.48	0.14	0.29	+ 1.02 — 0.18	+	47
W. C.	0.62	0.22	0.35	+ 1.42 — 0.22	+	33
S.	0.67	0.21	0.31	+ 1.65 — 0.27	+	31
Av.	0.56	0.17	0.30			

TABLE LXVI.

(*Poetry read as prose without punctuation marks*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. M. T.	0.45	0.12	0.26	+ 0.85 — 0.16	+	19
G. F. A.	0.51	0.16	0.31	+ 1.12 — 0.24		44
B. S. G.	0.66	0.19	0.28	+ 1.50 — 0.20	±	32
Av.	0.54	0.16	0.28			

TABLE LXVII.

(*Poetry read as poetry, Browning verses*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. M. T.	0.52	0.18	0.34	+ 1.09 — 0.16	+	48
C. O.	0.45	0.16	0.35	+ 1.06 — 0.16	±	47
Av.	0.48	0.17	0.34			

Table LXVIII.

(*Poetry read as poetry, Tennyson verses*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
W. C.	0.66	0.20	0.30	+1.52 —0.22	+	33
S.	0.77	0.21	0.27	+1.47 —0.25	+	32
Av.	0.71	0.20	0.28			

Table LXIX.

(*Summary of complex-centroid intervals of verses of Browning and Tennyson read as poetry*)

Table.	*a*	*p*	$\frac{p}{a}$
LXVII.	0.48	0.17	0.34
LXVIII.	0.71	0.20	0.28
Av.	0.59	0.18	0.31

Table LXX.

(*Prose read as poetry*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
S. I.	0.61	0.17	0.27	1.25 —0.32	+	21
O. E. S.	0.69	0.20	0.29	1.37 —0.21	+	23
Av.	0.65	0.18	0.28			

Table LXXI.

(*Summary of complex-centroid intervals speech*)

Table.	*a*	*p*	$\frac{p}{a}$
LXIII.	0.55	0.13	0.22
LXIV.	0.62	0.21	0.32
LXV.	0.56	0.17	0.30
LXVI.	0.54	0.16	0.28
LXIX.	0.59	0.18	0.31
LXX.	0.65	0.18	0.28
Av.	0.58	0.17	0.28

Unit of measurement, 1^s.
a, average duration.
p, immediate probable error.
$\frac{p}{a}$, relative immediate probable error.
e, extreme intervals.
c, character of greater extreme.
n, number of intervals.

Results.—The number of intervals measured was 1054.

The duration of a given complex-centroid interval in speech will probably fall somewhere within the limits of 0.14^s (= shortest in series, J. M. T., Table LXV.), and 1.93^s (= longest in series, W. L. P., Table LXIV.), a range of about $1\frac{3}{4}^s$. The range for the average interval of different records is about $\frac{1}{4}$ of this (from 0.32^s, J. K., Table LX. to 0.75^s, A. D. $B._A$, Table LIX.), and for the averages of different sets of records less than $\frac{1}{7}$ (from 0.47^s, Table LX. to 0.71^s, Table LXVIII.). The highest range between the averages of the selections read twice by the same person was 0.10^s (S.); the lowest, 0.03^s (C. O.).

The extreme deviations of these intervals from the average were always of the nature of excesses. In the measurements 100% were plus extremes. This may be due more to the pauses than the brevity of the average. The former enables the range to be extended almost indefinitely, while it can be only slightly abbreviated. The extremes for the different sets, may be obtained in the *e* and *c* columns of the tables.

The length of the average complex-centroid interval is a little over half a second (0.58^s).

The averages in prose (0.59^s) and poetry (0.57^s) are practically equal in length. The tendency, just barely perceptible, is to make the intervals of prose the longer. This conclusion may only hold for such intervals as were measured, which were prevailingly 2-syllable sound- and 1-pause-2-syllable composite-intervals.

The longest average in any record was 0.07^s shorter in prose (0.70^s, W. L. P.), than in poetry (S., Table LXVIII.); and the shortest average was 0.10^s shorter in the reading scansion of English poetry (C. O., Table LXVII.) than in prose (0.55^s, G. A. A.).

The intervals composed regularly of two syllables (0.71^s = average for set of Tennyson verses; 0.66^s, same for sing-song and doggerel scansion) are longer than those composed of a mixture of one, two, three, four and five syllables, provided the number of 1-syllable intervals is relatively larger than the number of 3-, 4- and 5-syllable intervals.

The average inequality of the lengths of complex-centroid intervals in all varieties of speech was about 0.17^s, or 28% of the length of the average.

The regularity in a given set of records of a variety of verses of poetry of the predominantly 2-syllable type, scanned according to the four types of scansion, was about 8% higher than that for the intervals in a given number and variety of sentences of prose, uttered in various ways. The irregularity for the former was 22% (Tables LIX., LXI., LXII., LXIX.); of the latter, 30% (Tables LXIV., LXX.).

The regularity in a given set of records of English poetry, the intervals of which are mostly of the 2-syllable pattern, and the scansion of

which is rhythmically free (reading scansion, Table LIX.) was about 3% higher than that in a corresponding set of records of English prose, the reading of which is rhythmically free (32%, Table LXIV.). When the scansion or reading is natural, artistic and rhythmically free, the complex-centroid intervals are only slightly more regular in poetry than in prose.

The most regular coordination of the intervals in prose (e. g., C. O. S., Table LXIV. ; and Table LXX.) was superior to the coordination in many records of poetry.

The most regular coordination in poetry occurs in routine scansion ; it is never perfect. The lowest percentage in seventeen records was 4% (Table LXII. ; E. H. $T._B$). Beating the time with the finger increased the regularity about three and a half times.

The intervals in the most regular scansion of poety were about six and one-half times more regular than in the most regular reading of prose. Excluding the record of routine scansion with regulative concomitant, this is reduced to one and six-sevenths.

The coordination in doggerel and in the most regular specimen of reading scansion, was about equally good (19%, O. S., Table LXI., and E. W. S., Table LIX.). In routine scansion it was about twice as regular as this (9%, Table LXII.).

In sing-song scansion it was slightly poorer than in doggerel scansion, but better than in reading scansion in general. Reading scansion represents the highest degree of irregularity of any form of scansion.

The test records of the verses of Tennyson and Browning indicate that the highest regularity in reading scansion occurs when the intervals consist of a uniform mode of composition or the same number of syllables (cf. Tables LXVII. and LXVIII.). The difference in the regularity of the renderings of the tests is small. The absence of the punctuation marks did not decrease it.

The range of irregularity for the records of a given number of persons will be about 0.27^s (from 0.02^s, routine scantion, Table LXII. to 0.29^s, reading scansion, J W. R., Table LIX.), or 38% (0.04 to 0.42, reading scansion, Japanese poetry, I. M., Table LX.). The range for the averages of the sets was 25% (from 9%, routine scansion, to 34%, verses of Browning read as poetry, Table LXVII.) ; and for the same persons repeating the same selections 7% (from 1% to 8%, in both cases, J. M. T.).

With a standard of 10% of inequality, the complex-centroid intervals are rhythmically coordinated in about 6% of the records of poetry and in none of those of prose. With 15% as the standard none of the

latter and 12% of the former satisfy the demands of rhythm (this is exclusive of the test records read as prose, both with and without punctuation marks).

The above items for the individual records (test records, records of foreign languages, etc.) may be obtained in the tables.

Measurements of simple-centroid intervals.

Table LXXII.

(Reading scansion, English poetry)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. W. R.	0.63	0.11	0.17	+1.12 −0.33	+	91
E. W. S.	0.44	0.05	0.11	+0.62 —0.25	—	74
A. D. B. $_A$	0.47	0.12	0.25	+0.79 —0.27	+	11
A. D. B. $_B$	0.60	0.06	0.10	+0.81 —0.27	+	9
W. W.	0.41	0.10	0.24	+0.97 —0.18	+	64
A. R. P.	0.43	0.06	0.14	+0.57 —0.27	—	11
Av.	0.50	0.08	0.17			

Table LXXIII.

(Reading scansion, Japanese and Persian poetry)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. K.	0.32	0.06	0.18	+0.52 −0.16	[illegible]	12
I. M.	0.34	0.10	0.29	+0.60 —0.18	—	13
K. H. K.	0.53	0.08	0.15	+0.68 −0.31	—	12
Av.	0.39	0.08	0.20			

Table LXXIV.

(Sing-song and doggeral scansion)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
H. Ö.	0.50	0.04	0.08	+0.55 −0.47	+	21
O. S.	0.59	0.04	0.06	+0.78 —0.47	+	21
Av.	0.54	0.04	0.07			

TABLE LXXV.

(*Routine scansion*)

Subject	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
E. H. $T._A$	0.47	0.06	0.12	+0.70 —0.31	—	12
E. H. $T._B$	0.48	0.01	0.03	+0.52 —0.43	—	12
Av.	0.47	0.03	0.07			

TABLE LXXVI.

(*Summary of simple-centroid intervals of poetry*)

Table.	*a*	*p*	$\frac{p}{a}$
LXXII.	0.50	0.08	0.17
LXXIII.	0.39	0.08	0.20
LXXIV.	0.54	0.04	0.07
LXXV.	0.47	0.03	0.07
Av.	0.47	0.06	0.13

TABLE LXXVII.

(*Prose*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
G. A. A.	0.44	0.10	0.22	+0.70 −0.20	[illegible]	51
W. L. P.	0.44	0.12	0.27	+0.83 −0.23	+	36
C. O. S.	0.54	0.11	0.20	+0.80 −0.20		42
Av.	0.47	0.11	0.23			

TABLE LXXVIII.

(*Poetry read as prose*)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. M. T.	0.37	0.10	0.27	+0.68 —0.14	+	34
C. O.	0.36	0.08	0.22	+0.62 −0.18	+	31
W. C.	0.44	0.07	0.15	+0.60 —0.22		22
S.	0.50	0.06	0.12	+0.75 −0.27	[illegible]	20
Av.	0.41	0.08	0.19			

Table LXXIX.

(Poetry read as prose without punctuation marks)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
G. F. A.	0.38	0.08	0.21	+0.60 −0.24	+	29
B. S. G.	0.49	0.11	0.22	+0.75 −0.20	—	17
J. M. T.	0.36	0.10	0.27	+0.72 −0.16	+	32
Av.	0.41	0.09	0.23			

Table LXXX.

(Poetry read as poetry, verses of Browning)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
J. M. T.	0.38	0.09	0.23	+0.75 −0.16	+	48
C. O.	0.30	0.06	0.20	+0.50 −0.16	+	31
Av.	0.34	0.07	0.21			

Table LXXXI.

(Poetry read as poetry, verses of Tennyson)

Subject.	*a*	*p*	$\frac{p}{a}$	*e*	*c*	*n*
W. C.	0.48	0.09	0.18	+0.65 −0.22	—	21
S.	0.55	0.09	0.16	+0.85 −0.25	0	18
Av.	0.51	0.09	0.17			

Table LXXXII.

(Summary of simple-centroid intervals of verses of Browning and Tennyson, read as poetry)

Table.	*a*	*p*	$\frac{p}{a}$
LXXX.	0.34	0.07	0.21
LXXXI.	0.51	0.09	0.17
Av.	0.42	0.08	0.19

Table LXXXIII.

(Prose read as poetry)

Subject.	a	p	$\frac{p}{a}$	e	c	n
S. I.	0.46	0.07	0.15	−0.70 −0.32	−	13
O. E. S.	0.53	0.13	0.24	+0.90 −0.21	+	15
Av.	0.49	0.10	0.19			

Table LXXXIV.

(Summary of simple-centroid intervals of speech)

Table.	a	p	$\frac{p}{a}$
LXXVI.	0.47	0.06	0.13
LXXVII.	0.47	0.11	0.23
LXXVIII.	0.41	0.08	0.19
LXXIX.	0.41	0.09	0.23
LXXXII.	0.42	0.08	0.19
LXXXIII.	0.49	0.10	0.19
Av.	0.44	0.08	0.19

Unit of measurement, 1^s.
a, average duration.
p, immediate probable error.
$\frac{p}{a}$, relative immediate probable error.
e, extreme intervals.
c, character of greater extreme.
n, number of intervals.

Results.—Eight hundred and twenty-three measurements were made.

The duration of a given simple-centroid interval of speech will presumably lie somewhere between 0.14^s (J. M. T., Table LXXVIII.) and 1.12^s (J. W. R., Table LXXII.), a range of about 1 second. The range for the averages of a given series of records will be about $\frac{1}{3}$ of this (from 0.30^s, C. O., Table LXXX., to 0.63^s J. W. R., Table LXXII.); and for those of different sets, about $\frac{1}{5}$ (from 0.34^s, verses of Browning read as poetry, to 0.54^s, sing-song and doggerel scansion). The highest range for the selections repeated by the same person was 0.06^s (C. O.); the lowest, 0.01^s (J. M. T.). For the complex intervals J. M. T.'s range was the highest; here it is the lowest. He seems most irregular in his pauses.

The longest average for any *set* of prose records was 0.49^s (Table LXXXIII.): for poetry, 0.54^s (Table LXXIV., sing-song and doggerel scansion). The shortest for poetry was 0.34^s (Table LXXX., Browning verses). The longest average for any *record* was 0.54^s for prose (prayer,

C. O. S., Table LXXVII.), and 0.63^s for poetry (J. W. R., Table LXXII.); the shortest for prose, 0.44^s (G. A. A., Table LXXVII.), and for poetry, 0.30^s (C. O., Table LXXX.). The range seems larger in poetry.

In 69% of the records the largest deviations from the average were those of excess; in 27%, of deficiency; and about 3% were equal.

The average duration of these intervals for different kinds of speech is somewhat below half a second (0.44^s). This corresponds closely with MEUMANN's period (0.40^s) for purely subjective rhythmisation.

The averages for a variety of sentences of English prose (0.48^s) and a variety of verses of English poetry (0.46^s) are practically equal. The tendency, barely perceptible, is towards a longer interval in prose. The intervals in the poetry measured were predominantly 2-syllable.

The average is longer in verses consisting uniformly of 2-syllable intervals, than in verses consisting of a haphazard alternation of 1-, 2-, 3-, 4-, and 5-syllable intervals, provided that the number of 1-syllable is higher than the number of 3-, 4-, and 5-syllable intervals. The average for the verses of Tennyson was 0.51^s; for those of Browning, 0.34^s (Table LXXXII.). The latter contained numerous 1-syllable intervals. So did those of Byron where the average was 0.41^s (W. W., Table LXXII.).

The inequality in the lengths of these intervals in various kinds of speech is about 0.08^s, or 19% of the average length.

The regularity for a given series of records of a variety of poetical verses of the predominantly 2-syllable type of intervals, scanned according to the four types of scansion will be about 8% higher than that for records of a variety of prose sentences spoken in various ways (21%, av. irregularity of Tables LXXVII. and LXXXIII.). This is exclusive of the Japanese and Persian records. For records of English verses composed mainly of the 2-syllable pattern, the scansion of which is rhythmically free, it will probably be about 6% higher than for records of English prose, the rendering of which is rhythmically free (irregularity 23%, Table LXXVII.).

The most regular coordinations in prose (15%, S. I.; 20%, C. O. S.) may often be higher than some coordinations in poetry (cf. A. D. $B._A$, W. W., J. M. T., C. O., etc.).

The intervals in the most regular type of scansion of poetry (E. H. $T._B$, Table LXXV.) were about five times more regular than in the most regular reading of prose (15%, S. I., Table LXXXIII., melodious prose). Excluding the record of routine scansion with regulative concomitant, the regularity was two and a half times greater.

The most regular coordination of the intervals in poetry is never perfect. The lowest observed percentage of irregularity in seventeen records was 3%.

The highest regularity occurred in the type of routine scansion accompanied by beating time. This type represents the acme of regularity in the accentual or centroid rhythm of speech.

The coordination in sing-song (irregularity, 8%) and doggerel scansion (irregularity, 6%) may be approximately the same. Both are fully twice as irregular as mechanically regulated (routine) scansion.

The most regular coordination in reading scansion (10% of irregularity, A. D. $B._B$) is slightly inferior to the coordination in sing-song scansion. The regularity in reading scansion is the most inexact of all the types of scansion of poetry. It is most exact, other things equal, when the intervals are composed quite uniformly of the same number of syllables (contrast Tennyson's verses with Browning's and Byron's). The regularity in the reading of the selections as prose and poetry was the same. When the punctuation marks were eliminated, it was only slightly increased.

The range in the irregularity of the intervals in various kinds of speech may be about 0.11^s (from 0.01^s, routine scansion, to 0.12^s, A. D. $B._A$, poetry, and W. L. P., prose), or 26% (from 3%, routine scansion, to 29%, Japanese poetry). The range for the different sets was 16% (from 7%, Tables LXXIV. and LXXV., to 23%, Tables LXXVII. and LXXIX.), or about ⅔ of the former (the individual records); and for the repeated readings by the same person, 2% (from 2%, C. O., to 4%, J. M. T. and S.).

According to the 10% standard, 23% of the records of poetry are rhythmical, exclusive of records of poetry read as prose, both with and without punctuation marks. With a standard of 15%, 46% of the records of poetry, and 20% of the records of prose (record of melodious prose), are rhythmical. Even in the latter case, more than one-half of a fairly comprehensive collection of speech records was non-rhythmical.

Comparison of complex- and simple-centroid intervals.

The range for the former was approximately 1.80 times that for the latter.

The general character of the extreme deviations was the same for both. For the complex, however, they were uniformly, for the simple chiefly excess extremes.

The average for the complex was 0.14^s, or 1.31 times, longer than for the simple intervals.

The averages of both were very slightly longer in prose than in poetry. This is probably due (1) to the composition of the intervals. In the prose records the longer intervals (3-, 4-, and 5-syllable) were relatively more numerous. (2) The tendency in poetry, especially in routine and doggerel scansion, to rhythmize the pause; this limits its length.

Both intervals had the longest averages in verses consisting of the uniform 2-syllable type.

The coordination was about 1½ times more regular for the simple than for the complex.

The difference was less between the regularity of the simple intervals in poetry and prose, than between the regularity of the complex in poetry and prose.

For the reading scansion of English poetry and prose, the difference was twice as large for the complex as for the simple.

In the most regular type of scansion, the regularity was slightly higher for the simple than the complex. The beating of time increased the regularity of the former most. Relatively to other types of scanning, however, it greatly increased the regularity of the complex intervals.

The range of irregularity for a variety of speech of different persons was about 1½ times larger (1.46) for the complex than for the simple intervals.

With a provisional standard of 15%, the simple intervals were rhythmically coordinated in about four times as many records of poetry as were the complex.

General deduction.—The most perfect manifestation of centroid rhythm in speech never reaches the zero point of irregularity. This point is most nearly approached in simple-centroid intervals of uniform composition, mechanically scanned.

Bearing on the rhythmic function of the pause.—The facts reached regarding the duration and regularity of sound and composite, and single and complex intervals necessitate a revision of the prevalent notion of the pause as compensatory,[1] as the rhythmical equivalent for a missing syllable or a foot. POE[2] so regards the cæsura. We have seen that the introduction of the pause uniformly disturbs the rhythm of the centroids. The records without exception show this. The coordination of the complex intervals was almost without exception so irregular as to defy rhythmisation. In only two records does the inequality fall to 15%,

[1] LANIER, The Science of English Verse 189, New York 1880.
GUEST, A History of English Rhythms, 77, London 1882.

[2] POE, The Rationale of Verse, Works, VI 89, Chicago 1895.

while for the simple intervals, there are eight such records of poetry and one of prose. The pause constitutes the one rhythmical; the other, in the main, non-rhythmical. It creates two alternating, though interpenetrating, sequences within the unity of the discourse; the one consisting of expiration intervals, being rhythmical, the one consisting of expiration and vacant intervals, being non-rhythmical.

We saw that the terminal pauses, which were longer and more regular than the sectional, unified the verses. The tables for the sectional pauses and the simple and complex intervals show that when the number of sectional pauses is large the coordination of the intervals is poor. When they occur quite regularly in long verses these are split up into two. Most of those reading the Browning verses written as prose who made regular sectional pauses, when asked to write the sentences as verses, doubled the number. Here the pauses segregated the verses into two parts. Both the terminal and sectional pauses may thus become rhythmically recurrent, provided they recur at fairly regular intervals and are of fairly regular length.

Rhythmically, pauses check the continuity of the centroid rhythm of speech; they limit the length of centroid sequences. This makes centroid rhythm essentially *discontinuous*, an alternately interrupted and recommencing flow, an inter-pause rhythm. Mechanical rhythm, *e. g.* clock ticks, consists of continuous sequences of coordinated intervals.

Pauses also assist in imparting a felt unity to sequences of several centroid intervals, provided they follow a certain law, whence arises a secondary rhythm, that of vacant and expiration intervals. Thus we get the verse interval rhythm.

Psychologically, the pause serves to avert the monotony of long sequences of regularly recurrent stimuli. The length of these will affect the quality of the rhythm. Too long or too short series are objectionable. This may differ for different kinds of scansion and rhythmisation. In subjective rhythmisation a continuance of clicks for 45 seconds has been found favorable, for 70 seconds disgusting.[1] The pause thus contributes change, variety, relaxation, and also a complex feeling of rhythm due to the involution or interpenetration of the two orders of rhythm.

Bearing on some phases of speech rhythm.—(a) *The elements of rhythm.*—Speech rhythm has been defined as a "law of succession" (GUEST); a "principle of proportion introduced into language," "inferior meter" (SEELEY and ABBOTT); "periodic stimulation of sounds or of a small group of sounds" (GURNEY); "the succession and involution of unities, that is, unities within unities, applicable to feet, verses and stanzas"

[1] TITCHENER, Experimental Psychology, I II 340, New York 1901.

(CORSON); "a system of accentuation" (HEGEL); "a succession of tones in various length and shortness, following in a certain time form" (LOBE); "the coordination of speech sounds in time and duration" (LANIER). In these definitions are involved two theories.

1) The "*time theory*," holding that the element of time is fundamental, and emphasis subordinate. The sounds of a rhythmical sequence must, above all things, be coordinated, periodical. LANIER[1] ("simple time relations"), GURNEY[2] ("a fixed scheme of recurrence"), BOLTON[3] (regular recurrence the most important element; "accentuated sounds form a secondary rhythm out of the primary"), HAUPTMANN,[4] WESTPHAL,[4] LOBE,[4] HERBART,[4] LOTZE,[4] SCHOPENHAUER,[4] and EDWARDS,[5] may be cited as representatives.

(2) The "*accentualist theory*," making emphasis the essence, and time a secondary element. Some of the exponents are GUEST[6] ("accent the sole principle," "rhythm of accent independent of quantity"), HEGEL[7] ("rhythm a system of accentuation"), GUMMERE,[8] and KOSTLIN[9] ("change of accents of the tones").

It is hard to pin the writers down to any one statement, but the positions seem to be as indicated. When accent is made primary the element of time becomes, at best, merely "a regulative principle or a principle of embellishment." The opposite rigorist holds that no number of accents, nor manner of variation of the elements, can render sounds rhythmical unless they are strictly periodic.

The centroid theory of speech rhythm emphasizes the truth contained in both of these theories. No recurrence of centroids can be rhythmized unless the length and regularity of the intervals fulfil the requirements of the rhythmical time-sense. This may differ slightly for different individuals. It may allow of slight grades of rhythm, bad, good, excellent. At a certain point, however, the rhythmic perception or feeling ceases entirely.

The criterion of regularity may also differ for the different kinds of rhythms, sensory and motor, speech, music, walking, dancing, etc. For routine scansion, with the attention focused on the regularity, the crite-

[1] LANIER, The Science of English Verse, III., New York 1880.

[2] GURNEY, The Power of Sound, 439, London 1880.

[3] BOLTON, *Rhythm*, Am. Jour. Psych., 1893 VI.

[4] MEUMANN, *Untersuchungen zur Psychologie und Aesthetik des Rhythmus*, Philos. Stud., 1894 X 250.

[5] GUEST, A History of English Rhythm, 108, London 1882.

[6] GUEST, as before, 108–110.

[7] HEGEL, Aesthetic, trans. of Kedney, 257.

[8] GUMMERE, Handbook of Poetics, 137, Boston 1885.

[9] MEUMANN, Philos. Stud., 1894 X 250.

rion may be higher than for reading scansion, where the best expression of thought is essential. A variation (+ and —) of $\frac{1}{60}$ of empty intervals between clicks 4.27ˢ in length was always correctly noticed in the experiments of HALL and JASTROW;[1] one of $\frac{1}{120}$, nearly always.

These requirements, however, give only subjective or objective periodicity. Grouping may be temporally perfect or periodic (Lanier's primary rhythm), and still not be psychologically rhythmical. To constitute perceived rhythms, some member of the groups must be accentuated, at least subjectively. Walking may be periodic; it is not normally rhythmical. It becomes rhythmical when one step is made more energetic, or when attention is given to it, or a sound is uttered or heard (*e. g.*, a drum) as it descends.

EBBINGHAUS[2] found that the coordination of the lengths of syllables spoken loud was impossible without differences of accent: no rhythmising of syllables mechanically uttered was possible. In repeating the numerals 1–10 regularly, I could feel no rhythm until some numbers were subordinated, i. e., formed into centroid intervals. Then grouping soon became inevitable. A trained vocalist sang the vowel ō into the phonograph without interruption for nearly 40ˢ. He was told to make it perfectly smooth, without modification in intensity and pitch. The resulting sound was fairly smooth at the beginning; after about 10ˢ oscillations in the intensity were noticeable. Speech as a motor phenomenon is necessarily centroidal.

Emphasis is unavoidably imposed upon certain members of a sound sequence. Numerous experiments[3] show that these are always formed into groups because of subjective emphasis.

The form of these groups will depend upon representative factors arising from the experience of the subject; they will conform to some represented rhythm (counting, beating time, clock beats, pendulum swings, puffs of the engine, etc.).

In BOLTON'S[4] experiments the normal, easiest or psychologically prior form was the 2-group (first syllable accented, trochee) and 4-group. The 3-group (dactylic form easiest) and 2 + 3-group were readily suggested, but not the 5-group. HALL and JASTROW[5] found that in *counting* a series of sounds or clicks 3- and 4-groups must be farther apart than

[1] HALL AND JASTROW, *Studies of rhythm*, Mind, 1886 XI 61.

[2] MEUMANN, Philos. Stud., 1894 X 423, 424.

[3] BOLTON, Am. Jour. Psych., VI 145–238.
EBBINGHAUS and DIETZE, in MEUMANN, Philos. Stud., 1894 X 423.

[4] BOLTON, as before, 212, 216, 222.

[5] HALL and JASTROW, *Studies of Rhythm*, Mind, 1886 XI 58, 61.

2-groups. Twenty-four to forty clicks per second were easily distinguished by the average ear [limit of ability to discriminate sounds = $\frac{1}{132}$ (HELMHOLTZ) or $\frac{1}{500}$ (EXNER) of a second]. Counting, however, involved naming of the clicks. The number-names are not of uniform ease and length. This influenced the grouping.

Thus the centroid, or the element of emphasis, is always present in speech. Consciousness is centroidal. Perceived rhythm is a highly regular alternation of intense and lesser intense states of consciousness.

(b) *The nature of rhythm.*—It is a series of mental events involving affection, intellection and conation. The rôle of the first has been stated by WUNDT[1] (feeling of strain and expectation, of unity, and a certain degree of emotion), by SMITH[2] (rhythm a progressive emotion with coordinated motor discharges), by MEUMANN[3] (æsthetic feelings) and by LIPPS[4] (associative æsthetic feelings). The latter are probably such as proportion, symmetry, unity in variety, harmony, euphony, beauty, sublimity. The emotions may be those of different degrees of pleasantness, ecstasy, joy, vivacity, erethic diathesis, orgasm, love, hate, sweetness, well-feeling, etc.; and perhaps disagreeableness—monotony, insipidity, sadness, discomfort, disgust, etc. The sense feelings may include smoothness, assonance, dissonance, intense or weak, long or short, fast or slow, excitations, freshness or tiredness, muscular activity or innervation, tingling, heightened temperature and blood pressure, etc.

MEUMANN[5] (rhythm a perception) emphasizes the rôle played by intellection. This is best illustrated by the change of grouping produced by ideation (association factor). This factor will largely determine the character of the affectional response of the individual.

The present researches emphasize the factor of *conation:* without subjective emphasis, the centroid, no rhythm. Intellective perception alone may perceive periodicities, but not rhythm; for this conative perception, or intermittent attention, aroused and sustained by feeling, is necessary. *Felt motor* innervations are fundamental to rhythmic perception. They need not necessarily be *movements* or vaso-motor discharges, but motor impulses (probably only mental beats). Rhythm is a peculiarly *felt motor perception* of movement in time.

[1] WUNDT, Outlines of Psychology (Eng. tr.), 167, 176, Leipzig 1897.

[2] SMITH, Philos. Stud., 1900 XVI 291.

[3] MEUMANN, Philos. Stud., 1894 X 264.

[4] LIPPS, *Aesthetische Einfühlung*, Zeit. für Psychol. u. Physiol., CXXII 441.

[5] MEUMANN, as above, 272, 284.

VI. Comparison of intervals in speech.

Tables of average number of syllables per centroid and expiration interval, and the interval of the unit of measurement (1^s).

Table LXXXV.

(*Poetry*)

Subject.	*c*	*s*	*e*
E. W. S.	2.21	4.22	6.42
J. W. R.	2.06	2.75	8.77
A. D. B.	2.15	2.90	3.73
W. W.	1.74	3.01	4.89
A. R. P.	2.00	3.73	6.00
H. Ö.	2.09	3.16	5.81
O. S.	2.10	3.10	7.80
J. M. T.	1.56	2.95	
C. O. $_B$	1.56	3.40	4.41
W. C. $_B$	1.90	2 90	4.26
S. $_B$	2.00	2.56	4.26
G. F. A.	1.65	3.13	4.56
B. S. G.	2.00	2.99	4.00
Av.	1.92	3.14	5.41

Table LXXXVI.

(*Prose*)

Subject.	*c*	*s*	*e*
G. A. A.	2.53	4.48	4.48
W. L. P.	2.20	3.14	6.05
C. O. S.	2.24	3.53	6.57
S. I.	2.71	4.38	6.33
O. E. S.	2.47	3.54	6.33
Av.	2.43	3.81	5.95

c, complex centroid intervals.
s, intervals of 1^s.
e, expiration intervals.

Results.—The average number of syllables for each complex centroid interval of all varieties of speech was 2.17.

For the records of poetry it was 1.92; for those of prose, 2.43, the latter being a trifle over 25% larger than the former. For verses composed predominantly of the 3-syllable type of intervals, this relation would probably be reversed. Since there is no orderly arrangement of interval pattern in prose, the variation, while it may be constant and sometimes large, will in the average be small (cf. records of Table LXXXVI.). The range between the highest and lowest average for

prose was 0.51 syllable; for poetry, 0.65 syllable. The range for the individual intervals was from 1 to 5 syllables. A centroid interval in speech may thus range from a 1-syllable to a 5-syllable.

The average number of syllables per second of time for all kinds of speech was 3.47; for the records of poetry, 3.14; for those of prose, 3.81, or about 20% more than for poetry.

The range for the averages of prose was 1.34 syllable per second (from 3.14 to 4.48); for poetry, 1.66 syllable (from 2.56 to 4.22), or about 23% larger than the range for prose.

On the basis of the general average, the number of syllables uttered per minute in speaking is approximately 208. This involves an equal number of changes in the action of the vocal cords. The most rapid contractions in aspirating *t* and *k* has been found to never exceed 6 double or 12 single contractions per second.[1]

The average number of syllables per expiration interval for all kinds of speech was 5.68; for poetry, 5.41; for prose 5.95, or 10% more than for poetry.

The range for the averages of prose was 1.85 syllable; of poetry, 5.04, or nearly two and three-fourths times longer than the former.

This range applies only to the averages of ordinary expiration intervals, and not to those of maximum expiration intervals. The following test was made: The subject, after having inhaled as much air as the lungs would comfortably hold (attempting to make each inhalation equal) read as many of a variety of short and long words as was possible without replenishing the original supply of air. The test was stopped when fortuitous inhalations occurred. The tendency was to increase the rate as the context became familiar and practice set in. All the utterances were considerably more rapid than those in ordinary speech. Sometimes this increased the number of syllables: frequently it produced a more rapid expenditure of air, and decreased the number. Several relatively slow readings contained the largest number.

These were the average number of syllables for seven or eight tests each of 11 subjects: 122, 147, 148, 153, 163, 165, 187, 190, 230, 257, and 276 The general average is 182.5. The highest average came from a trained vocalist. Most subjects gained up to a certain point and then fell off. The lowest percentage of gain was 19 syllables; the highest 119. The range is 154 syllables (from 122 to 276).

Compared with the average normal interval, the average maximum contains approximately 32 times as many syllables. The range of a breath group may thus extend from 1 to about 175 syllables. The normal is

[1] HALL AND JASTROW, *Studies of rhythm*, Mind, 1886 XI 59.

the stock-in-trade of ordinary speech. The maximum is perhaps never reached in speech. The nearest approach is found in impassioned speech.

Comparison of intervals.—The average interval of the second was 0.60 syllable longer than the complex centroid interval, and 0 63 syllable shorter than the expiration interval. For the prose records, it was 0.56 syllable longer than the centroid interval, and the same shorter than the expiration interval. For poetry it was 0.63 syllable longer than the centroid, and 0.72 syllable shorter than the expiration interval.

The expiration intervals of poetry were 2.81 syllables longer than the centroid intervals of poetry and prose respectively.

The range between the longest and shortest averages of the tables was: for the centroid interval, 0.58 syllable; for the second, 1.45; and for the expiration, 5.04. The average range for the second was 2.50 syllables longer than for the centroid, and 4.75 syllables shorter than for the expiration interval.

The difference between the range of syllables for the second and centroid interval was over 4 times the difference between the average number of syllables for each; and the same for the second and expiration interval was 7.5 times the difference between the averages for each.

Character of the greater extreme.—This was for all the intervals of speech in the vast majority of cases of the nature of excess. In no case does the percentage fall below 59%.

The extremes were relatively least frequent in the verse intervals, and most frequent in the complex, vacant, expiration and simple centroid intervals in the order named. The converse order represents the grade of frequency of the deficiency extremes.

Only in the expiration and simple centroid intervals were there any equal extremes, the percentage in both cases being very small (6% and 3%).

May we say that the longer the average of the interval the smaller, and the shorter the larger, is the percentage of excess extremes? The average vacant and simple centroid intervals are equal in length, yet differ in the percentage of plus extremes. The complex are longer, yet have no *minus* extremes: all are *plus*. The simple intervals are considerably shorter than the expiration and complex, yet have a higher percentage of *minus* extremes than these—more than double as many as the vacant intervals.

Hence, other things being equal, the extremes of deficiency are relatively most infrequent and those of excess most frequent, in intervals containing pauses.

Range of duration for single intervals.—Taking the range for the simple centroid (0.08^s) as the norm, the range for the vacant was 1.37 longer, for the complex 1.82, for the verse 4.20, and for the expiration intervals 4.72. The difference between the longest (4.65^s) and the shortest range is 3.57^s.

The range for the average of the complex intervals is 1.30 times that of the simple ($=0.33^s$); for the vacant 1.54 times; for the expiration 5.00 times; and for the verse 10.5 times. The longest range (3.47^s) is 3.14^s longer than the shortest. The order for the individual intervals and the averages is seen to differ. Both fall into two groups, the one a small, the other a large, range.

Average duration.—Taking the average of the simple centroid as the norm (0.44^s), the average for the vacant was of the same length; for the complex, 1.31 times longer; for the composite, 2.20 times longer; for the expiration, 2.70 times longer; and for the verse intervals, 6.11 times longer.

With the sum of the averages as the unit, the verse interval constitutes 42%, the expiration 19%, the composite 15%, the complex 9%, and the vacant and simple centroid 7% each, of this unit. The average for the verse intervals is about 2¼ times longer than for the expiration.

The proportion between the averages of the expiration and vacant intervals, or between the quantity of sound and silence in speech, was as 2.71 to 1. Apparently about 73% of the time consumed in speaking is utilized in sound production and in exhalation, and 27% in pausing. Most pauses probably represent an inhalation.

The proportion of sound to silence was slightly larger for poetry than for prose. The difference is negligible. The largest percentage in English poetry for the expiration intervals was 79% (J. W. R. record); the smallest, 57% (W. C.).

The average for the composite intervals is nearly two and a fourth times larger than that for the vacant and simple. Hence the sounds in the composite intervals occupy more time than the silence.

All the above relations of the intervals in the various records, may be obtained in the tables.

Range of irregularity.—*In units of time,* with the range of the simple intervals (0.11^s) as the norm, that of the complex and vacant is each 2.45 times larger, of the verse 4.18 times and of the expiration 7.81 times. As a fraction of the average duration the range for the simple (26%) is 2½ times that for the verse; and for the complex 1.46, for the vacant 1.65 and for the expiration 1.69 times that for the simple intervals.

Relative regularity.—The average for the verse intervals is 2.11 as regular as for the simple (19%); for the latter 1.47 times as regular as for the complex, 1.52 as regular as for the vacant, and 1.78 as regular as for the expiration intervals.

Percentage of records of rhythmically coordinated intervals.—Applying a 10% standard of irregularity, it is as follows: For verse, 59% ; simple centroid, 14% ; expiration, 12% ; vacant, 8%, and complex centroid, 4%. The manner of coordination differs for the intervals. The expiration and vacant were picked out wherever they occurred in the records. The one intervenes between the other; the tables do not include the measurements of the two in one series. The verse intervals are likewise separated by terminal pauses. These are not included in calculating their rhythm.

Bearing on metrical problems.—If meter requires a relatively perfect coordination of intervals, what conditions are essential to fulfil this requirement? What type of reading and of interval most frequently and fully satisfies the demand?

Types of meter.—We have seen that there are two types of speech intervals which, on the basis of the criterion of regularity, may with scientific precision be called rhythmical, viz., the verse and simple centroid intervals.

As affects the simple centroid intervals, the measurements showed that the criterion of metrical perfection was most highly approached in poetry. The conditions for the highest regularity in poetry were: (1) the distribution throughout the verses of centroid intervals consisting of the same number of components; and (2) the scanning of the verses according to the routine type of scansion.

As affects the phrase intervals, the measurements showed that the criterion of metrical perfection was approached only in the verse intervals of poetry, the number of whose centroids was more limited and regular than in the sentences of prose. The conditions for securing the highest regularity, other things being equal, were: (1) limiting the duration of the intervals; and (2) subordinating the sectional to the terminal pauses, and the sectional to the terminal centroids, of the intervals.

Meter par excellence—choice of types.—Which of the two types is prior, primary or fundamental? The question is complicated. Neither type exists in isolation; both rhythms interpenetrate. A good quality of verse-interval rhythm will largely depend on, or perhaps demand, a smooth flow of centroids. It may also be associated with pause rhythms, sectional and terminal. Remembering these cautions, which type has, on the whole, the stronger claim?

Hints from the measurements.—These indicated that the range of irregularity was about 2½ times larger, the average irregularity over 2 times larger, and the number of records of unrhythmical intervals about 4 times more for the simple intervals. In only one record was the variation of the intervals less for the simple centroid. From the point of view of regularity, experiment uniformly emphasizes the importance of the verse interval type.

This type, however, to become a rhythmic unity requires a sort of "verse" centroid, as distinguished from the "foot" centroids, at the beginning or the end. The complicated relations of the verse and foot centroids give rise to a hegemony of centroids. The verse as a rhythmical whole might be changed by eliminating the sectional and especially the terminal centroids. It could remain a regular, yet not rhythmical, group.

Hints from the test.—The grouping of the sounds into visible verses produced a delusion: spontaneous, unsuspecting thought did not distinguish verse sequences from sentence sequences. With words arranged like prose sentences the verses were pronounced non-rhythmical sentences of prose. With words arranged like verses the prose sentences were declared rhythmical, and to be poetry.

In these judgments the smoothness or rhythm of the word flow was a factor. The determining factor was the form of print, or a felt verse meter. Some who read the Tennyson verses as prose pronounced them rugged but still poetry, because of a felt verse interval rhythm. The appeal to unsuspecting thought also shows the importance of this form of rhythm.

Hints from the poet.—The following questions, bearing also on other aspects of the metrical problem, were submitted.

Questionnaire on the art of versification in respect to meter.

Describe the attitude which you assume, in the act of poetical production, toward the versification of the poem that you are about to write.

I.) (a) Do you determine in advance the particular kind of meter which you intend to follow? (b) Do you decide that a given poem shall consist of a certain kind and number of measures—dactyls, spondees, iambi, anapests, etc.—to the line? (c) If so, do you go over your verses to see that they invariably follow the metrical system adopted? (d) Or are there forces unconsciously operative preventing any deviation from a chosen norm?

Or do you predetermine the number of emphatic, or accented syllables, *i. e.*, stress-points, irrespective of the number of unaccented or weak syllables, for the verses of your poem?

II.) Or, on the other hand, is the measure in your poetry a subordinate factor? Does it sustain a relation to special moods and emotions and to the character of the

thought which you would express? Do you become so inspired by the thought to be expressed that the form never for a moment rises consciously before you? Your foot-scheme would thus be the result of a species of unconscious cerebration, and you would become aware of its character not before, but either in the very act of poetical creativeness, or upon subsequent examination of your completed product.

(III.) Do you feel a demand for meter? Whence does it spring?

(IV.) If you employ both methods, what relation do they sustain in reference to (a) frequency; (b) ease or naturalness, and (c) results, metrical and poetical?

The meter of Christabel, it may here be noted, was founded on the principle of "counting in each line the accents and not the syllables."[1] Each verse contains four accents; the number of syllables may vary from seven to twelve. Tennyson's lyric, "The Poet's Mind," may be consulted.

Replies.

(English)

Ella Wheeler Wilcox: I cannot fully analyze my methods. They are not always the same. The idea comes usually first, a sentence, a word, or a thought only—like a point of light. I may carry it about for weeks—the germ of the poem—shapeless and undefined. When I take time to sit down and focus my mind upon it, the poem comes.

Sometimes I begin two or three shapes before I fix upon the style of verse; oftener I begin the poem as it remains, the lines falling into rhythm themselves.

Again, I decide upon a sonnet form, the moment the idea comes. One of my best sonnets ("A Minor Chord") came that way. I met two nurses on the street and the line "The wistful unkissed mouths of nurses" came to me. I knew it was a sonnet line, and days afterward sat down and built the sonnet to fit that line.

In talking with a very excitable man one day about single tax (that is, *listening* while he talked), he said: "No question is ever settled until it is settled right." I said, "Oh there is a poem in that," and at once sat down and wrote it—it has been used in two political campaigns by the Bryan orators.

Sometimes without any idea, a rhythm, a wordless song, goes ringing through my head. I walk to it—I work to it—and it pursues me until I find an idea to put into the measure.

At other times, without idea or measure, I settle down, knowing something will come.

—— —— ——:[2] My own poetic mental habit is lyrical. As nearly as I can remember, each poem, or theme, or motif (as one would say in music) occurs to me almost simultaneously in both thought and form—a poetic phrase (made up of words in a certain poetic accent and diction) shapes itself in the mind. I do not realize at the moment what the meter is. I may, or may not, realize what the stanza or complete poem-form is to be. I think that most of my lyrics have occurred to me "on the road"—when moving about, going back and forth to my office, travelling, sometimes when I am reading. Sometimes a line or two will rest in my mind for years, and add other lines spontaneously; it may be in distant scenes. For instance, for years I had in my mind these lines, descriptive of the glow above New York as seen at night from the Staten Island ferry:

Lies like a lily white
On the black pool of night.

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[1] COLERIDGE, Poetical Works, I 188, Aldine Edition.

[2] The writer wishes his name reserved.

Years afterward, while at Stratford-on-Avon for the first time, the whole poem came to me in a shape that I had never had in mind at all, so far as I know. It was not at all affected in form by the memory of Shakespeare verse forms, as might have been natural in the circumstances.

AT NIGHT.

The sky is dark, and dark the bay below
Save where the midnight city's pallid glow
    Lies like a lily white
    On the black pool of night.

O rushing steamer, hurry on thy way
Across the swirling Kills and gusty bay,
    To where the eddying tide
    Strikes hard the city's side !

For there, between the river and the sea,
Beneath that glow,—the lily's heart to me,—
    A sleeping mother mild,
    And by her breast a child !

My busy life has given me little time to build poems ; hence their brevity and subjective quality. They start, with few exceptions, from a personal experience and emotion. At times in my mind a line or verse seems to sing itself (not an actual *tune* but a series of poetic accents) ; hardly anything at first except a sense of verbal music without words, stirred by the delight, perhaps, in something startlingly beautiful in nature.

In the volume "In Palestine" you will find a poem, "How to the Singer Comes the Song," which seems to be itself an answer to your question. After a few phrases or lines have started into being something seems to say, this is to be a poem in such or such a conventional form—blank verse, four lines with rhymes, six, a sonnet, etc.,— or else in some new form altogether, with stanzas irregular or unconventional in themselves but made regular by following succeeding stanzas all of the same form, or else with the regular irregularity of the dithyrambic form. After the form is thus established everything is bent to moulding the poem according to the conventional or sought-out, or accidental pattern. I find the line is in such a form—well then, it must be true to that form—unless with some variation that cannot be charged to carelessness, but to deliberate intention or deliberate adoption of an accidentally pleasing form. At one time—virtually the beginning of my poetical writing—I was very much stirred by the Italian sonnets, by Shakespeare's, Milton's, etc., and my poetic impulses naturally flowed into sonnet form ; the seed phrase in the mind was apt to be inevitably sonnet-wise. I have written four in a day. I have no doubt that while Dante was writing his triology his poetical thought fell naturally into the terza rima.

I think the initial thought is generally a *line ;* rhymes usually follow with lightning speed. I believe this is because my mind—like the minds of all moderns—is full of the music of rhymes. If I had been an ancient Hebrew, or a verse-maker of some other primitive race in the early days, the music in my mind would doubtless have been rhyme, less—would have fallen with apparent spontaneity into the forms prevalent in the language of the time.

—— —— ——:[1] (I.) I do not determine in advance the stanza forms to be used in any particular poem, unless a thought or feeling occurs to me which seems fitted for

[1] The writer wishes his name reserved.

one of the more elaborate, recognized stanza forms, like the sonnet, rondeau, Spenserian stanza, etc. In that case I enter upon the construction of it consciously. I do go over my verses to see that there are the right number of lines, that they rhyme in the right order, and I do not commonly count the number of syllables or accents in a line in composing isometric verse. The form once adopted—say the ten-syllable iambic—my ear keeps count of the syllables half-consciously in the act of composition. When the length of the line varies, however, sometimes I find that I have made a mistake—have got a line in that is a foot too short or too long—and so have to correct.

(II.) The adoption of a line as the unit of a verse-composition, is commonly the result of unconscious cerebration, I think; or rather, perhaps of unconscious imitation, my memory being full of verses of all kinds. It is with some half-remembered measure that a thought or feeling in my mind tends to associate itself, giving possibly a line or two lines which emerge definitely into consciousness. Then the end-word in this line or lines, suggests a rhyme-word and so a stanza or couplet is formed—usually in the middle of the poem that is to be. From this, by a conscious effort of construction, I build up the piece, before and after, on the norm thus established.

(III.) I do feel a demand for meter, but could not say whence it springs without theorizing about it. Certain emotions, not at the moment they are experienced, but afterwards, when recalled, seem to crave metrical expression. A few words come to me metrically arranged, and that is the nucleus of a poem if I carry the thing any further by a definite effort of will. If not, they simply drop out of memory in a day or two. I have sometimes dreamed a line or two.

(IV.) I have not noticed the relative frequency. The two methods run together and are confused in any actual piece of verse-composition.

James Riley: (I.) I know nothing of the anatomy (I will call it) of poetry. My own experience after nineteen years, writing my first poem at the age of thirty-three, is that the form strengthens and meets unconsciously the requirements of the poem, in accordance to the inner growth.

(II.) This question answers all I know of poetry. It seems to me the poet should write his moods as they come to him, and later complete or reject the work according to its worth. In my dialect poems—in the New England and Irish dialects—I have always known what I was going to write,—to a great extent before writing a line. They have generally been of actual occurrences in my life. On the other hand—and it has always been puzzling to me why it should be so—I have written a great number of stories in prose, and in them I can never find myself able to insert a single actual incident in my experience.

To show how erratic is the poetic mood as it comes to me sometimes, out of which if worked out comes a poem, I mention a poem written a year ago and which appeared in the "New England Magazine" of last May. I remember the words and thought that led to the opening lines of the poem:

> "After the book is written, after its page is read,
> After the soul has brought and left what to the age is wed,"—

What connection it has with the poem I am unable to tell. The lines were to me a music that led to other music, and allowing the second feeling to express itself, I began striking the words on the typewriter "over and over and over." I did it with no connection of any thought but that should in some way involve and be of life. In this way the music came first. My first writing—beginning of the poem—was with the thought

that every stanza should begin as the first, and I can see now that the thought would have been higher, if the refrain had so continued.

N. M. Nelson: In my few metrical compositions, I have never stopped to think whether I used dactyls or spondees, or counted the syllables until I had finished. I simply let my thoughts assume the form suited to their expression. When in such a mood, the metrical part is secondary to thought, but may still be in strict conformity with poetical usage.

There is certainly a necessity for meter. This may be because we unconsciously imitate poetical composition, or the mind may require meter in certain moods to express inspired thought.

E. L. Fox (Yale '02): (I.) (a) Before beginning to write, yes; but not before the spirit and general idea of the poem are clearly defined in my mind. (b) No. (c) No. (d) Yes. The spirit of the poem generally swings itself into a suitable meter, so that choice of a meter is usually unnecessary (when writing "to order" on a subject I am not enthusiastic about, this is not true). Sometimes I hit upon a meter I like; it sticks in my mind and suddenly occurs to me as just the thing for a subject I have been thinking over for a week or so.

(II.) The measure is distinctly paramount. It sustains a close relation to the character of the thought. The last sentence applies perfectly to my experience.

(III.) Yes; from the nature of the theme and the enthusiasm which it excites.

(IV.) I employ but one method.

Paul F. Gilbert (Yale '01): My ear tests the quality of meter. Minor defects are corrected by mechanical examination. Forces unconsciously operative prevent with some reliability any eccentricity. The number of accented syllables is predetermined, subject to modification, with little attention to specific forms of verse, the number being settled by their capability of expressing the nucleus of the poem and other favorite sentiments which may be afterwards suggested.

My moods and emotions are emphasized by the employment of a suitable verse form, but this form is not arbitrary.

The expression of spontaneous thought often runs into the channels of least resistance—usually some elementary form of verse. The demand for meter arises from a natural desire for repetition. Repetition is agreeable, the abrupt disagreeable.

The practice of determining in advance the meter and the number of accented syllables tends to produce artistic results; but I find the spontaneous expression more satisfactory. The imitation of established verse forms leads to artificiality.

(Norwegian)

Wilhelm Petersen: (I.) (a) Sometimes I do determine the meter in advance, *e. g.*, when I write new words to some old melody, either from choice, or by the request of others. In such cases the song known to me in that meter has a certain influence on what I write, perhaps through mood-association.

(b) Strictly speaking, I do not think I ever determined in advance (except as above stated) any precise meter, as dactyl, spondee, iambus, anapest, etc. I do think, however, that after my thought has assumed a certain rhythm (the original poetic form, an emotion-product) I develop it in consciousness into a more regular meter, partly by choice influenced by my acquaintance with prosody and literature, partly as it may happen to fall in with the completed "motif" that is to form the basis of my poem.

(c) That depends. If I desire to make a "finished product," I follow the laws of prosody and rhetoric in detail.

(d) No, I hardly think there are. My opinion is that a person working under the influence of the soul-forces in a subconscious state, simply follows the rhythmic fall of his thought as accentuated by the underlying mood-waves, and the suitable words fall into the place demanded either by a musical force in subconsciousness, or are called forth simultaneously with the thought. The result is: accented-tone syllables and stress-points. Meter and rhyme are secondary results due to conscious effort toward art-form.

(II.) When the writer is unconscious of imitation and uninfluenced by mood-association, "measure" in poetry must be subordinate in degree and secondary in time, and is rather an art-effect than a factor. As the "motif" in original inspired production is "emotion of soul fused into thought"—eine Regung der Seele durch Gesinnung bewutz und im Gedanken fliezend gemacht—the desire for the materialization of which demands from the soul a conscious effort in language production, and as, furthermore, language follows the most rigid laws, just as lines do in painting and sculpture, I cannot quite see how there can exist any direct relation between the creative and conceiving mood and the outward art-form that the complete poetical thought finally assumes. That some relation may exist, it were rash to deny. The natural steps in the development of a poetical thought would then, it seems to me, be as follows: Underlying emotion, or inspiration, moving towards and shaping itself into a "motif" that rises through subconscious effort and determines "rhythm" or "tone-accent," and flowing into word-expression, following in the form it finally assumes, the laws of prosody as to meter, and of rhetoric as to choice of words.

(III.) Yes. It springs partly, at least, from the nature of the language used. However, language itself has been influenced by this demand, indicating that the demand for meter *may* follow from the first the steps in the above scheme of development, although not consciously sought for till the last.

(IV.) I think I have written (a) mostly without predetermined measure, (b) far more easily and naturally then, and (c) with better results, metrically and poetically.

(Swedish)

Jakob Bonggren: While sitting, walking, or, though rarely, lying down, some thought or, oftener, picture flashes into my mind, which I conclude will make a good poem. Sometimes I hear a meter or some line of poetry in connection with it, and that is the nucleus of the poem about to be begun.

(I.) If the mind only begets a thought or picture, I try to find a meter; this is usually an easy matter. I think I can *hear* what is the proper meter, so I seldom have difficulty in selecting a form. I very seldom, if ever, decide from the beginning the number of lines in the poem; it grows until it is full-grown. If I have one verse, it is easy to get all the following or preceding ones molded in the same form. I read through the poem, correcting and changing the words here and there, but very seldom the meter.

If I have a thought as a nucleus, I sometimes study out some of the strongest points and stress-words, select a meter, and work out the poem. Poems worked out in this way (from pictures or thoughts) I have found to be the best.

(II.) Whenever I am called upon to write a poem on a certain subject I slowly and carefully work it out; select a suitable meter and think out all I want to include in the poem. After handling my subject for a while, I sometimes feel warmed up, and then everything is easy. There seems to be an unconscious cerebration working along with conscious thought and will, when something worthy the name of a poem is created. The "poems" written to order I have found to be generally poor, unless I have warmed up in the act of composing.

(IV.) Answers as above (I.) and (II.).

As bearing on the present question, these statements indicate that both types of language rhythm, varying more or less with different composers, are felt, and make a demand upon the mind that conceives poetical thoughts and embodies itself in metrical language. Witness the terms: "wordless song," "poetic accents," "verbal music, without words," "measure paramount;" and "poetic phrase," "sonnet form," "verse form," "verse line singing itself," music of the lines, the line as the "unit of verse-composition," as the "initial thought," the "seed phrase," feeling that the line must be "true to its form," and "measure secondary."

Deduction.—Three lines of evidence thus uniformly emphasize the importance of the verse interval type of meter in speech rhythm. These intervals may approximate better than the centroid intervals the larger fluctuations of attention in speaking, which alone may possess the power to genuinely attune and cadence the soul. May we therefore regard them as the primary or chief type of the rhythm of speech?

It is a pleasant duty to acknowledge my obligations to the subjects, to those answering the questionnaire, and to the editor of the Studies.

NOTE.

The price of all volumes of these Studies is fixed at \$1 each. For cash with order the price is 75 cents, provided no correspondence or other clerical labor is involved.

STUDIES

FROM THE

Yale Psychological Laboratory

EDITED BY

EDWARD W. SCRIPTURE, PH.D.

Director of the Psychological Laboratory

1902

VOL. X.

YALE UNIVERSITY

NEW HAVEN, CONN.

PRESS OF
THE NEW ERA PRINTING COMPANY
LANCASTER, PA.

CONTENTS

RESEARCHES ON RHYTHMIC ACTION

BY

ISHIRO MIYAKE, PH.D.

The present investigation of rhythmic action was begun in 1898 and ended in 1901.

I. ARHYTHMIC ACTION.

The term 'arhythmic action' is used here to mean a series of movements executed at intentionally irregular intervals. In the following experiments we have to observe how irregularly such a series of movements can be performed.

In the preliminary experiments I used the LUDWIG kymograph and two MAREY tambours, so arranged that the recording point of one of the tambours drew a line on the smoked surface of the cylinder of the kymograph.[1] The subject was required to hold the lever connected with the other tambour between his thumb and index finger, and, his eyes being closed, to move it up and down successively at irregular intervals at a rather rapid rate.

The experiments were made on three subjects; a specimen record is shown in Fig. 1. The height of the curve corresponds to the amplitude

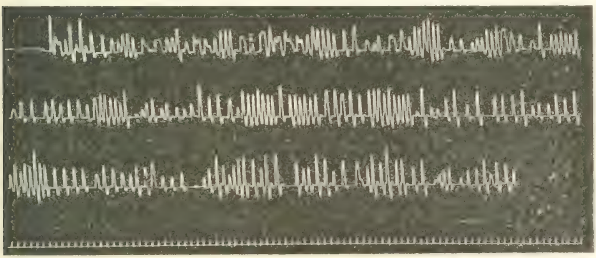

FIG. 1.

of the movement, and therefore to the intensity of the exerted muscular energy, while the horizontal distance indicates the length of the time

[1] Details of the arrangement are given in Exercise VIII of SCRIPTURE, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 108, Fig. 16.

between the successive movements. The line at the bottom indicates fifths of a second. It was observed in this record as well as in the others: (1) that there is constantly recurring tendency to repeat equal intervals in succession; (2) that the same intensity of the muscular energy is also often repeated; and (3) that the weak and strong intensities often alternate. The attempt at irregular action thus shows a persistent tendency to revert to action regular in time and intensity.

The records obtained in the above experiments show the characteristics of arhythmic action under the various circumstances for the various subjects. Accurate measurements of the length of the intervals can be better obtained by another method. A Deprez marker and a key with a break contact were put in series in a 1^{am} current. The pointer of the marker was rested lightly against the smoked surface of the cylinder of the kymograph. The subject was asked to tap the key at intervals as irregular as possible, the slowest speed of the two successive beats being limited to about one second. He was seated comfortably before the appa-

Record I. Subject S.

1	2	3	4	5	6	7	8	9	10	11	12
55	36	27	23	14	13	13	39	32	27	28	11
13	14	15	16	17	18	19	20	21	22	23	24
12	12	12	12	12	15	12	19	27	14	14	12
25	26	27	28	29	30	31	32	33	34	35	36
14	13	14	15	13	12	14	22	12	11	17	20
37	38	39	40	41	42	43	44	45	46	47	48
22	34	31	38	59	15	18	12	40	33	30	40
49	50	51	52	53	54	55	56	57	58	59	60
14	15	14	15	13	15	15	16	14	14	14	15
61	62	63	64	65	66	67	68	69	70	71	72
14	16	34	34	32	28	19	10	15	15	30	15
73	74	75	76	77	78	79	80	81	82	83	84
14	14	15	27	15	15	32	17	22	14	14	16
85	86	87	88	89	90	91	92	93	94	95	96
15	16	16	15	32	15	15	15	15	15	16	15
97	98	99	100	101	102	103	104	105	106	107	108
15	15	16	16	15	15	15	34	26	25	15	16
109	110	111	112	113	114	115	116	117	118	119	120
14	15	14	14	15	15	15	15	15	14	20	16
121	122	123	124	125	126	127	128	129	130	131	132
16	22	16	11	14	11	42	24	42	18	14	16
133	134	135	136	137	138	139	140	141			
19	14	17	20	19	17	16	32	56			

Unit of measurement, $\Sigma = 0.01^s$. First line indicates the serial number of the period. Second line indicates the length of the period in hundredths of a second.

ratus and his eyes were closed during the experiments. The speed of the kymograph was made fast enough so that the interval between the two successive beats could be measured with sufficient accuracy.

For obtaining the time line the JACQUET graphic chronometer was used, with the pointer vibrating five times a second; one-fifth of a second being divided into twenty equal parts, the one-hundredth part of a second could be used as a unit of measurement.

The subjects of the experiments were as follows: S, an instructor at Yale University; U, a student; and J, the steward of the psychological laboratory.

The results of the experiments are given in the accompanying RECORDS, in which the figures in the first horizontal line indicate the serial number of the period and those in the second horizontal line the length of the successive periods, $\Sigma = 0.01^s$ being the unit.

RECORD II. SUBJECT J.

1	2	3	4	5	6	7	8	9	10	11	12
31	14	14	12	12	12	22	16	23	16	54	12
13	14	15	16	17	18	19	20	21	22	23	24
38	14	14	15	14	29	10	16	18	32	15	13
25	26	27	28	29	30	31	32	33	34	35	36
20	38	18	60	14	16	16	16	16	16	16	34
37	38	39	40	41	42	43	44	45	46	47	48
15	34	34	34	15	17	17	18	18	16	33	16
49	50	51	52	53	54	55	56	57	58	59	60
20	16	16	16	46	16	14	16	15	16	45	14
61	62	63	64	65	66	67	68	69	70	71	72
16	55	16	19	14	30	18	16	23	22	42	42
73	74	75	76	77	78	79	80	81	82	83	84
18	18	18	14	24	44	17	30	18	38	16	16
85	86	87	88	89	90	91	92	93	94	95	96
18	14	16	14	14	14	13	14	16	16	16	28
97	98	99	100	101	102	103	104	105	106	107	108
16	30	34	19	16	14	14	40	42	18	34	46
109	110	111	112	113	114	115	116	117	118	119	120
14	16	16	16	16	12	14	16	15	14	14	16
121	122	123	124	125	126	127	128	129	130	131	132
18	33	16	20	16	14	14	18	22	15	16	15
133	134	135	136	137	138	139	140	141	142	143	144
14	14	15	14	14	20	13	20	19	11	11	16
145	146										
12	30										

Unit of measurement, $\Sigma = 0.01^s$. First line indicates the serial number of the period. Second line indicates the length of the period in hundredths of a second.

RECORD III. SUBJECT U.

1	2	3	4	5	6	7	8	9	10	11	12
12	13	20	20	16	16	16	16	10	10	16	19
13	14	15	16	17	18	19	20	21	22	23	24
12	22	11	12	22	31	37	20	12	17	16	16
25	26	27	28	29	30	31	32	33	34	35	36
15	15	15	14	13	10	18	40	40	27	16	30
37	38	39	40	41	42	43	44	45	46	47	48
46	35	40	12	18	19	27	25	30	25	40	32
49	50	51	52	53	54	55	56	57	58	59	60
14	20	28	33	27	35	30	29	38	32	14	15
61	62	63	64	65	66	67	68	69	70	71	72
15	15	13	17	32	37	38	20	18	12	30	30
73	74	75	76	77	78	79	80	81	82	83	84
26	25	15	36	35	40	23	26	33	45	30	13
85	86	87	88	89	90	91	92	93	94	95	96
20	20	14	14	15	16	15	15	14	28	48	28
97	98	99	100								
15	20	28	33								

Unit of measurement, $\Sigma = 0.01^s$. First line indicates the serial number of the period. Second line indicates the length of the period in hundredths of a second.

The records show the following facts: (1) there are repetitions of equal or about equal periods; (2) the unequal periods, which occur after or in the middle of a group of the repeated equal periods, are, in many cases, simple multiples of the latter; (3) the periods from 12 to 17 are most frequent; (4) rhythmic alternations of long and short intervals also occur.

These facts seem to indicate that arhythmic movements have a constant tendency to become rhythmic, notwithstanding the voluntary effort of the subject to execute the movements at irregular intervals. The subjects of the experiments invariably agreed in confessing that the arhythmic tappings required strenuous effort and that the performance was very fatiguing.

II. INFLUENCE OF AUDITORY AND VISUAL SENSATIONS ON RHYTHMIC ACTION.

The purpose of the experiments in this section was to determine how the regularity of the rhythmic movement is influenced by the auditory and visual stimuli.

As there are two different forms of rhythmic action, the one "regu-

lated" and the other "free,"[1] the experiments were made separately on both forms.

A. Regulated rhythmic action.

The function of the auditory or visual stimuli in regulated rhythmic action is to give us the objective standard of the period in coincidence with which we execute our movements. Of the two kinds of sensations, auditory and visual, which one is the more favorable for the regularity of the movements?

It was very important that the movements of the finger should be registered without producing any sound audible to the subject. A special key was made of a small piece of steel spring about 55^{mm} long and 4^{mm} in width fastened tightly at one end to a wooden block. A platinum point put on the block made contact with a similar point on the spring. A slight touch of a finger at the free end of the spring wire would break the electric circuit at the platinum point. Used inside a muff, the very faint click which it produced was rendered imperceptible to the ear.

For producing the auditory stimuli a pair of discharging points connected to a spark coil were used. The two brass rods were put in a horizontal line with a distance of about 2^{mm} between them and connected to the poles of the secondary circuit. When the primary circuit was broken a sound was produced by the spark. The points were put behind a black screen, so that the spark could not be seen by the subject.

For producing the visual stimuli without sound a GEISSLER tube was connected with the secondary coil. In order that the faint sound produced in the tube when the current passes through it might be made inaudible, the tube was put in a wooden box with a glass window in front.

The discharging points and the GEISSLER tube were connected by means of a double switch to the secondary current of the spark coil.

A simple brass contact on a WUNDT contact apparatus[2] attached to the LUDWIG kymograph was used for breaking the contact of the primary current of the spark coil at a regular interval. The break of the contact produced either the sound of the spark at the discharging points or the flash in the GEISSLER tube. By turning the switch either of the two stimuli could be used as desired. A DEPREZ marker connected in series with the key in 1^{am} current recorded on the smoked surface the moment of the beat of the finger. A fork of 10 complete vibrations a second

[1] SCRIPTURE, *Observations on rhythmic action*, Stud. Yale Psych. Lab., 1899 VII 102.

[2] WUNDT, Physiol. Psychol., 4. Aufl. II 424, Leipzig 1893.

SCRIPTURE, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 127.

registered its vibrations by a pair of MAREY tambours.[1] Each of the waves could be readily divided into ten equal parts; this made it possible to use $\Sigma = 0.01^s$ for the unit of measurement.

The point where the stimulus occurred was carefully determined, and a "zero-line" was drawn by moving the drum up and down.

In many respects the arrangements resembled those used in Exercise XII of the Yale laboratory course.[2]

The subject of the experiment was seated in a silent room and was asked to beat time on a noiseless key in coincidence either with the sounds of the sparks or with the flash of light from the GEISSLER tube. The records were taken in a adjoining room. The accessory communication between the two rooms was made by means of telegraph sounders.

The two sets of the trials, one with the sounds and the other with the flashes, were always made on the same occasion, the order of the trials being changed alternately. The experiments were made on two subjects; of these D was a student, and S an instructor at Yale University. The time interval of the signals was one second.

The results of different series of experiments for a period of 1^s are shown in Tables I and II, in which the average constant error is the average deviation of the beats from the corresponding signals. When the former comes after the latter, it is marked with the positive sign, and when it comes before, it is marked with the negative sign. The average constant error is thus:

$$\Delta t = \frac{x_1 + x_2 + \cdots + x_n}{n},$$

where x_1, x_2, $\cdots$, x_n are the lengths of time by which the finger beat occurs *after* the stimulus (negative values indicating *before*). In the column headed "Number of measurements," the number of the beats recorded is given. The column headed "Number of positive deviations" shows the number of the cases in which the beats came after the signal and the column headed "Number of negative deviations," that in which the beats came before. The probable error, which is an indication of the regularity of the movements, is obtained by the formula:

$$p = \tfrac{2}{3}\sqrt{\frac{(\Delta t - x_1)^2 + (\Delta t - x_2)^2 + \cdots + (\Delta t - x_n)^2}{n - 1}},$$

where Δt is the average constant error, x the individual deviation, and n the number of measurements.

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[1] LANGENDORFF, Physiologische Graphik, 134, Leipzig und Wien 1891.

[2] SCRIPTURE, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 127.

It will be observed in comparing Tables I and II that the probable errors of the movements with the sounds are smaller than those with the flashes. With the subject D, the probable errors for sounds vary from 0.9^{Σ} to 2.2^{Σ} and for flashes from 1.2^{Σ} to 3.4^{Σ}. With the subject S the probable errors for the sounds vary from 1.0^{Σ} to 1.7^{Σ}, for the flashes from 1.2^{Σ} to 6.9^{Σ}. The averages of the probable errors are: for D, sounds 1.7^{Σ}, flashes 1.9^{Σ}; for S, sounds 1.3^{Σ}, flashes 3.2^{Σ}.

TABLE I.

Regulated rhythmic action with sounds at intervals of 1^s.

Subject.	Average constant error.	Number of measurements.	Number of positive deviations.	Number of negative deviations.	Probable error.
D	— 9.0	16	0	16	2.0
	— 6.4	16	0	16	1.4
	— 5.5	20	0	20	2.0
	— 5.5	20	0	20	0.9
	— 2.0	20	2	18	2.2
	— 1.9	20	5	15	1.9
	— 6.6	20	0	20	1.6
	— 2.5	20	2	18	1.6
S	— 12.4	16	0	16	1.7
	— 7.2	16	0	16	1.2
	— 5.2	20	0	20	1.2
	— 3.4	18	0	18	1.0
	— 5.7	20	0	20	1.2
	— 3.9	20	0	19	1.2
	— 7.1	20	0	20	1.2
	— 5.8	20	0	20	1.3

Unit of measurement, $\Sigma = 0.01^s$.

TABLE II.

Regulated rhythmic action with flashes at intervals of 1^s.

Subject.	Average constant error.	Number of measurements.	Number of positive deviations.	Number of negative deviations.	Probable error.
D	— 13.7	16	0	16	3.4
	— 5.6	16	0	16	1.3
	+ 0.2	20	10	6	1.2
	+ 0.2	20	2	17	1.3
	— 2.3	20	0	20	1.2
	+ 2.3	20	3	17	2.8
	+ 8.1	16	16	0	1.8
	— 4.8	16	0	16	2.0
S	— 3.8	16	11	3	6.9
	— 3.6	16	0	15	1.3
	— 7.9	20	0	20	1.5
	— 7.0	20	0	20	1.9
	— 7.7	20	0	20	2.1
	+ 0.5	20	12	8	5.4
	— 23.3	20	20	0	5.6
	— 5.4	20	20	0	1.2

Unit of measurement, $\Sigma = 0.01^s$.

The results show that the rhythmic movements regulated by auditory sensations are more regular than those regulated by visual sensations. It may be also noticed that most of the beats of the finger come before the sounds and also before the flashes, but more often in the case of sounds. With the subject D the beats with sounds come before the signals 143 times out of 152, with the flashes 108 times out of 144. With the subject S all 150 beats with sounds came before the signals, with the flashes only 63 out of 152. This fact agrees with JOHNSON's[1] experiments, in which it was observed that all his subjects anticipated the signals in beating time with the strokes of an electric sounder.

B. Free rhythmic action.

In regulated rhythmic action the sensations are produced by some external means, and the length of the interval is beyond the control of the subject. In free rhythmic action these sensations are produced as the accompaniment or consequence of the movement, and the length of the interval depends on the speed of the movement as chosen by the subject himself.

The present experiments on free rhythmic action were carried on in the two different series of trials: (1) free rhythmic action with and without auditory sensations, and (2) free rhythmic action with and without visual sensations.

1. Free rhythmic action with and without auditory sensations.

The experiment consisted in tapping on a noiseless key. The small "strap" key used in this experiment was made of an elastic brass strip, *B*, 46^{mm} long and 9^{mm} wide, mounted on a wooden block, *E*; a brass stop, *C*, kept the free end of the spring from rising more than 4^{mm} from the block. A slight pressure on the button, *A*, at the free end of the strap forced it nearer the block and broke its contact with the brass stop. Platinum points, *D*, were used to ensure good contact between the strap and the stop.

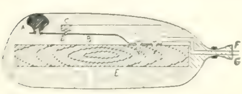

FIG. 2.

The key was put in a rubber bag and packed in felt so that the sound was rendered absolutely inaudible; the key was thus an absolutely noiseless one. The wires, *F*, *G*, projected from the bag. A spot on the surface of the rubber bag under which the button of the key was situated was marked with a sign. It indicated the point where the tapping was to be

[1] JOHNSON, *Researches in practice and habit*, Stud. Yale Psych. Lab., 1898 VI 51.

hand at what he considered to be a constant interval. The rate of the done. The adjustment of the key was such that the slightest touch broke the circuit.

The same discharging points that were used in the preceding experiments were fixed to two binding posts and mounted on a wooden block. All apparatus conditions were kept constant. The points were put behind a curtain so that the subject would hear the sounds of the sparks without seeing the flashes.

The general plan of the arrangement is shown in the accompanying diagram (Fig. 3). The noiseless key, *K*, with condenser, *C*, around the break was placed in the primary current, *P*, of a spark coil, *B*. The secondary coil, *S*, was connected in series with the metallic registering point of a PFEIL marker, *M*, and the discharging points, *J*, so that a break of the primary cir-

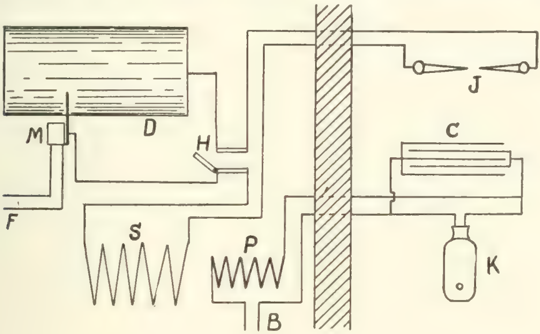

FIG. 3.

cuit would produce a spark on the time-line at the point of the marker and also between the discharging points at the same moment. In this way the movement of finger on the key, breaking the primary current, resulted in a sound of the spark between the discharging points and a record on a smoked drum, *D*, simultaneously.

A switch, *H*, was put in the secondary circuit around the discharging points. When the switch was closed, the short circuit prevented sparks at the discharging points, and the tapping on the key was not followed by the sound of the spark, although still recorded on the drum. A time-line was drawn on the drum by the marker, *M*, run by a 100 v. d. fork.

The key and the discharging points were placed in a special quiet room, the rest of the apparatus was in another room.

The subject with closed eyes beat time with the index finger of his right

movement was left entirely to his own choice. Two sets of the experiments, one with sounds and the other without sounds, were tried at the same time of day under as nearly the same conditions as possible. The order of trials of the two sets was alternated every time. From the record of one set of the beats 10 (in exceptional cases 9 or 8) successive periods were counted.

As an indication of the amount of the regularity of the movements, the probable error was used as in the preceding experiments. The formula for the immediate probable error is

$$p = \tfrac{2}{3}\sqrt{\frac{v_1^2 + v_2^2 + \cdots + v_n^2}{n - 1}},$$

where $v_1, v_2, \cdots, v_n$ are the differences between the individual measurements of the interval and the average, and n is the number of measurements. The relative probable error is

$$r = \frac{p}{a}$$

where a is the average; r indicates the relation of the amount of irregularity to the length of the interval.

The subjects of the experiments were four: M and Y, students of psychology; H, a student of law; and C, the laboratory mechanic.

Table III. shows the results of the experiments. For the sake of comparison, the results on the two kinds of movement, the one with and the other without sounds, are put side by side. The figures arranged in the same horizontal line are the results of the experiments taken on the same occasion. The unit of measurement was $\sigma = 0.001$.

A comparison of the corresponding probable errors in the same horizontal line will show that those of the free rhythmic movement with the sound are almost always smaller than those of the movement without the sound. This holds true for both the simple and the relative probable errors. In the case of the subject C the errors for the movement with the sound are always smaller than those of the movement without the sound. In the three other subjects M, H and Y, the case where the errors for the movement with the sound are larger than those of the movement without the sound occurs only once for each. The general conclusion may be drawn that free rhythmic movement with the sound is more regular than that without the sound.

It can be also noticed in the table that the length of the periods in general is shorter in the movements with the sound than in those without the sound. This is specially shown in the cases of the subjects

M and Y in which the periods with the sound are always shorter than those without the sound. This is probably due to the fact that the time interval marked off by the muscle, joint and skin sensations and the auditory sensations appears longer to the subjects than the equal interval which is marked off by the former group alone, and that therefore the subject tends to shorten the former.

TABLE III.

Free rhythmic action.

Subject.	With sound. Average time.	p	$\frac{r}{100}$	Without sound. Average time.	p	$\frac{r}{100}$
M	515	10.5	2.3	533	21.2	4.0
	523	11.2	2.1	559	19.6	3.4
	526	9.7	1.8	673	19.7	2.9
	500	13.8	2 7	528	7.0	1.3
	543	14.7	2.7	575	20.6	3.5
	519	11.3	2.1	550	22.0	4.1
	517	10.6	2.0	567	11.5	1.9
	502	4.7	0.9	523	14.8	2.8
H	428	10.4	2.4	360	18.5	5.1
	335	15.3	4.5	379	30.8	8.1
	349	15.0	4.3	388	14.0	3.6
	387	13.8	3.5	389	22.6	5.8
Y	803	14.8	1.8	856	29.5	3.4
	748	17.0	2.3	911	29.4	3.2
	1333	33.4	2.5	1347	42.7	3.1
	1305	32.6	2.5	1362	32.0	2.4
	709	25.3	3.5	713	28.2	3.0
	618	7.8	1.7	646	12.4	1.9
C	544	7.9	1.4	583	12.0	2.5
	561	9.3	1.6	581	16.6	2.8
	683	15.0	2.2	594	20.4	3.4
	791	12.0	1.5	703	20.8	2.7
	945	14.4	1.5	799	25.5	3.6
	958	14.2	1.4	1074	32.7	3.0
Average			2.3			3.4

Unit of measurement, $\sigma = 0.001^s$.

2. Free rhythmic action with and without visual sensations.

The apparatus and method of experimenting were mainly the same as before. The noiseless flash of the spark was substituted for sound. In order to render the sound of the spark inaudible, the discharging points were put in the inner of two concentric glass tubes; the sparks could be seen by the subject but not heard.

The results of the experiments are given in Table IV. The average of the intervals is, in general, the average of ten individual measurements, but

in exceptional cases the average of 8. The unit of measurement was $\sigma = 0.001^s$. The results of the two sets of the trials, the one with and the other without the flash of the spark, which were taken at the same time of the day, are put in the same horizontal line.

TABLE IV.

Free rhythmic action.

Subject.	With flash. Average time.	r	r 100	Without flash. Average time.	r	r 100
C	849	16.0	1.9	880	33.3	3.8
	929	27.3	2.9	883	13.3	1.5
	945	24.0	2.5	1187	35.3	2.9
	1150	32.7	2.8	1171	29.3	2.5
	1012	34.0	3.3	1049	25.3	2.3
	1044	19.0	1.7	1147	25.3	2.2
M	591	20.7	3.4	591	22.0	3.7
	537	16.0	2.9	572	9.3	1.6
	542	12.0	2.2	583	9.7	1.8
	563	17.0	3.1	573	1.7	2.9
	528	12.0	2.3	587	13.3	2.3
	540	12.7	2.6	583	18.0	3.1
	537	10.7	2.0	519	16.0	3.1
	514	20.7	4.2	536	19.3	3.4
	510	17.3	3.4	491	19.1	2.1
	504	18.0	3.6	504	15.3	3.0
Y	487	20.0	4.1	501	13.0	2.5
	569	14.9	2.6	521	13.3	2.5
	523	10.6	2.3	497	8.0	1.8
	490	15.3	3.1	481	12.0	2.7
	696	12.0	1.7	744	18.7	2.5
	642	18.0	2.8	691	11.3	1.6
	991	23.3	2.4	1038	28.7	2.7
	915	32.7	3.5	1008	21.3	2.1
	755	30.0	3.9	891	41.3	4.6
	792	16.0	2.0	715	21.3	3.5
	785	20.0	2.5	847	34.6	4.1
	764	22.0	2.0	721	27.3	3.8
Average			2.8			2.7

Unit of measurement, $\sigma = 0.001^s$.

A comparison of the corresponding relative errors in Table VII shows that in the two subjects C and Y, the number of cases is about the same in which the probable error is smaller for either one of the two kinds. The conclusion may be drawn that the presence of the flash of the spark with the beat of the finger does not affect the regularity of the rhythmic movement in a constant manner; sometimes the presence of the flash increases the regularity of the beats, but the reverse is quite as often true.

In the subject M, however, the number of the cases where the movement with the flash has the smaller probable error is only 3, while the

number of reverse cases is 6 out of 10. This result seems to indicate that the influence of the visual sensation on the regularity of the measurement is here not neutral, but active in destroying it.

III. Intensity and interval in rhythmic action.

The principal purpose of the following experiments was to determine how a change of intensity in the rhythmic movements affects the length of the interval.

Ebhardt,[1] working on the same problem, made two series of experiments. In the first series the tapping was done on an electric key, and in the second on a piano with electric connections. The records were taken in both cases on a kymograph. The results showed that the interval following the emphasized beat was lengthened as compared with that which followed the unemphasized beat. In Ebhardt's experiments the tapping was accompanied by the noise of the instrument.

A. Beats without noise.

The noiseless key described above (p. 8) was put with the Pfeil marker in series in a 1^{am} current. The metallic point of the marker was connected with one pole of the secondary coil of a spark coil, the other pole being connected to the base of the recording drum. The current from a 100 v. d. fork was sent through the primary coil. In this way the beats were recorded by checks in the line on the drum; these were divided by the sparks into equal spaces, each of which corresponded to $\frac{1}{100}$ of a second. The subject tapped with his finger (generally with the index finger of the right hand) on the noiseless key, according to the following schemes:

(*a*) 1′–2, 1′–2, 1′–2, ···
(*b*) 1–2′, 1–2′, 1–2′, ···
(*c*) 1′–2–3, 1′–2–3, ···
(*d*) 1–2′–3, 1–2′–3, ···

where the beat to be emphasized is marked with the sign ′. In the scheme 1′–2, for instance, the subject was asked to emphasize every first beat of the rhythmic group, but he had, at the same time, to try to keep always a uniform interval between two successive beats, not only between 1′ and 2, but also between 2 and 1′ although he was to think of the groups as in pairs 1′–2, not 2–1′. The speed of the movements was left to the choice of the subject.

[1] Ebhardt, *Zwei Beiträge zur Psychologie des Rhythmus und des Tempo*, Zt. f. Psych. u. Physiol. d. Sinn., 1898 XVIII 99.

The experiments were made on M. M., assistant in the psychological laboratory; C. W., a student of physics; and J. K., a student of philosophy.

Table V gives the results of the experiments on the scheme 1′–2. The averages of the intervals 1′ to 2 and 2 to 1′, obtained from each single series of experiments, are put in the same horizontal line. The unit of measurement is $\sigma = 0.001^s$. The ratio between the averages of the two intervals is given in the last column, the average for 1′ to 2 being taken as the unit.

The table shows that all the subjects made the interval 1′ to 2 longer than 2 to 1′: that is, the interval following the emphasized beat was made longer than that which follows the unemphasized beat.

The results of the experiments with the scheme 1–2′ are given in Table VI. It will be seen that the average length of the intervals 2′ to 1 is in all the cases longer than that of 1 to 2′.

A comparison of Tables V and VI shows the fact that the lengthening of the interval following the emphasized beat is more marked with the scheme 1–2′ than with 1′–2. The average ratios of the two intervals in the two different rhythmic schemes are:

	1′–2	1–2′
	1′ to 2 : 2 to 1′	1 to 2′ : 2′ to 1
C. W.	1.00 : 0.94	0.82 : 1.00
M. M.	1.00 : 0.93	0.90 : 1.00
J. K.	1.00 : 0.91	0.90 : 1.00

The relative lengths of the long and short intervals are not the same in the two different schemes; the interval which comes after the emphasized beat is comparatively longer in 1–2′ than in 1′–2. The same fact was observed by EBHARDT.[1]

Why is the interval following the emphasized beat lengthened more in one rhythmic scheme than in the other? This can be accounted for by assuming another factor, besides emphasis, that lengthens the period of the movements. It is due, as already pointed out by EBHARDT, to the formation of the rhythmic group. Rhythmic movements with grouping differ in their nature from those without grouping. The latter is merely a series of repeated movements at a uniform interval, in which every single movement is regarded as a coördinate unit. In the former, a series of the movements is divided into groups containing a certain number of movements as their content, and each of such groups is regarded as a unit.

EBHARDT supposed that at the end of the rhythmic group a suspension

[1] EBHARDT, *Zwei Beiträge zur Psychologie des Rhythmus und des Tempo*, Zt. f. Psych. u. Physiol. d. Sinn., 1898 XVIII 99.

Table V.

Beating on noiseless key.

Scheme : $1'$–2.

Subject.	Average of intervals from $1'$ to 2.	Average of intervals from 2 to $1'$.	Number of measurements.	Ratio $1'$ to 2 : 2 to $1'$.
C. W.	574	549	10	1.00 : 0.94
	595	589	10	1.00 : 1.00
	635	529	10	1.00 : 0.81
	538	503	10	1.00 : 0.98
	628	559	10	1.00 : 0.89
	641	632	10	1.00 : 0.99
M. M.	662	613	10	1.00 : 0.92
	601	578	10	1.00 : 0.96
	599	533	10	1.00 : 0.89
	633	614	10	1.00 : 0.97
	685	630	10	1.00 : 0.93
	708	670	10	1.00 : 0.99
J. K.	693	613	10	1.00 : 0.97
	716	681	10	1.00 : 0.96
	659	585	10	1.00 : 0.89
	602	536	10	1.00 : 0.89
	584	532	10	1.00 : 0.91
	669	654	10	1.00 : 0.83
	656	613	10	1.00 : 0.93
	647	593	10	1.00 : 0.92
	681	662	10	1.00 : 0.97

Unit of measurement, $\sigma = 0.001^s$.

Table VI.

Beating on noiseless key.

Scheme : 1–$2'$.

Subject.	Average time from 1 to $2'$.	Average time from $2'$ to 1.	Number of measurements.	Ratio 1 to $2'$: $2'$ to 1.
M. M.	549	573	10	0.96 : 1.00
	650	723	10	0.90 : 1.00
	682	746	10	0.92 : 1.00
	632	758	10	0.84 : 1 00
J. K.	661	747	10	0.89 : 1.00
	702	745	10	0.94 : 1.00
	575	634	10	0.90 : 1.00
	502	564	10	0.90 : 1.00
	582	706	10	0.83 : 1.00
	538	593	10	0.91 : 1.00
	579	588	9	0.98 : 1.00
	573	669	4	0.86 : 1.00
	557	585	10	0.95 : 1.00
	663	748	10	0.89 : 1.00
C. W.	597	661	10	0.90 : 1.00
	561	829	10	0.68 : 1.00
	618	773	10	0.80 : 1.00
	520	564	10	0.92 : 1.00
	665	721	10	0.86 : 1.00
	517	621	10	0.83 : 1.00
	548	736	10	0.75 : 1.00

Unit of measurement, $\sigma = 0.001^s$.

of attention takes place and that the moment of suspension can be considered as a dead time, which is to be added to the length of the foregoing group. We are not certain whether such suspension of the attention takes place or not. But it seems to be more probable that we have a tendency to insert some "pause" between two successive rhythmic groups, in order to mark off the groups distinctly from each other. The "pause" is to facilitate the formation of the groups.

We may suppose then that a certain length of "pause" was inserted between the groups in the scheme 1′–2 as well as in 1–2′, and that because the interval from 2′ to 1 of the scheme 1–2′ is lengthened both by the "pause" and the emphasis, it is made considerably longer than the time from 1 to 2′, whereas in the scheme 1′–2 the time from 1′ to 2 is lengthened only by the emphasis, while the time from 2′ to 1 is lengthened by the "pause," whereby the difference between 1–2′ and 2′–1 is not so great.

TABLE VII.

Beating on noiseless key.

Scheme 1′–2–3.

Subject.	Average time from 1′ to 2.	Average time from 2 to 3.	Average time from 3 to 1′.	Number of measurements.	Ratios 1′ to 2 : 2 to 3 : 3 to 1′.
M. M.	722	445	638	7	1.00 : 0.62 : 0.88
	790	462	780	10	1.00 : 0.58 : 0.99
J. K.	708	657	617	10	1.00 : 0.93 : 0.87
	724	655	650	10	1.00 : 0.90 : 0.90
	974	968	949	9	1.00 : 0.99 : 0.97
	757	739	752	10	1.00 : 0.97 : 0.99
	734	592	620	10	1.00 : 0.81 : 0.85
	663	646	634	10	1.00 : 0.99 : 0.96
C. W.	621	590	574	10	1.00 : 0.96 : 0.92
	624	614	621	10	1.00 : 0.99 : 1.00
	586	589	578	10	1.00 : 1.00 : 0.99
	601	595	572	10	1.00 : 0.99 : 0.95
	553	541	508	10	1.00 : 0.98 : 0.92
	559	546	550	10	1.00 : 0.98 : 0.95
	575	559	541	10	1.00 : 0.97 : 0.94

Unit of measurement, $\sigma = 0.001^s$.

The results of the experiments on the scheme 1′–2–3 are given in Table VII. The ratios of the average intervals were found here by assuming the time from 1′ to 2 as the unit.

The table shows again that the interval following the emphasized beat is longer than that which follows the unemphasized one.

The lengthening of the interval between the groups is not remarkable here. Although with the subject M. M. the interval 3 to 1′ is longer than 2 to 3, with J. K. the two intervals are about equal and with C. W. 3 to

1′ is in a majority of cases shorter than 2 to 3. This is due perhaps to the fact that for the last two subjects the "pause" between the groups was very short. Moreover, it is probable that the two subjects made the second member of the group stronger than the third, the beats being made not exactly in the scheme 1′–2–3, but in a manner something like 1″–2′–3 with the consequence that the interval following the second beat on account of the emphasis became longer than that following the third.

The results of the experiments on the scheme 1–2′–3 are given in Table VIII. The ratios of the average intervals were obtained by regarding 2′ to 3 as a unit. It will be observed here that the interval 3′ to 1 is very constantly longer than 1 to 2′.

TABLE VIII.

Beating on noiseless key.

Scheme: 1–2′–3.

Subject.	Average time from 1 to 2′.	Average time from 2′ to 3.	Average time from 3 to 1.	Number of measurements.	Ratios 1 to 2′ : 2′ to 3 : 3 to 1.
M. M.	371	714	668	9	0.52 : 1.00 : 0.93
	404	601	642	7	0.67 : 1.00 : 1.07
J. K.	689	719	713	10	0.96 : 1.00 : 0.99
	802	940	842	10	0.85 : 1.00 : 0.90
	761	862	806	9	0.88 : 1.00 : 0.93
	739	780	769	10	0.91 : 1.00 : 0.98
	685	740	765	10	0.93 : 1.00 : 1.08
	890	927	909	9	0.96 : 1.00 : 0.98
	704	730	762	10	0.97 : 1.00 : 1.04
C. W.	638	614	639	10	1.04 : 1.00 : 1.04
	730	744	733	10	0.98 : 1.00 : 0.98
	746	758	753	9	0.98 : 1.00 : 0.99
	567	581	466	8	0.98 : 1.00 : 0.80
	405	485	491	10	0.96 : 1.00 : 1.02
	515	517	525	10	0.99 : 1.00 : 1.02

Unit of measurement, $\sigma = 0.001^s$.

When 2′ to 1 is compared to 3 to 1, sometimes the latter is longer than the former, although in a majority of the cases the former is longer than the latter. This fact indicates that there is a strong tendency to lengthen the interval between the groups. If 1–2′–3 is compared to 1′–2–3, we find that there is a remarkable difference between the two rhythmic schemes in regard to lengthening of the intervals between the groups. The interval 3 to 1′ of the scheme 1′–2–3 is not so much lengthened as 3 to 1 of 1–2′–3. In other words the "pause" between the groups is longer in 1–2′–3 than in 1′–2–3. This fact indicates that the length of the "pause" is not the same in all rhythmic forms. It depends, probably, on

the amount of difficulty of the formation of the rhythmic groups. The more difficult the formation of the groups, the longer is the pause. In the case 1′–2–3 with the first beat of a group emphasized, the group can be easily marked off from the preceding or the following groups, and the rhythmic group can be formed, without lengthening very much the interval between them. But the case is different with the scheme 1–2′–3, where neither the first nor the last beat of a group is emphasized. Of the two similar beats one comes at the end of a group and the other at the beginning of the next group; the two successive groups can be marked off distinctly only by lengthening the interval between them.

B. Drum beats.

The same apparatus was used as before except that an ordinary snare drum 30^{cm} in diameter and 50^{cm} in height was substituted for the noiseless key. Both ends were covered with vellum. In order to make the electric connection with this instrument, the touch key (Fig. 4), mounted

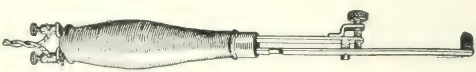

FIG. 4.

on a wooden block, was fastened to the outer wall of the drum, so that the rubber button of the key was in contact with the vellum of the lower side. When the drum was struck at the upper end, the movement of the lower end broke the electric contact of the key. The key was put in series with the PFEIL marker. The beat on the drum could thus be recorded on the smoked paper. The metallic point of the marker was placed in the secondary circuit of a spark coil whose primary circuit was interrupted by a 100 v. d. fork as in preceding experiment.

The subject was required to stand before the table on which the drum was put and to beat on it with a stick according to prescribed rhythmic schemes. For the beating the arm movement of the right hand was used instead of the finger movement of the previous experiments.

Table IX gives the results of the experiments with the scheme 1′–2. In obtaining the ratios of the average intervals the time from 1′ to 2 is regarded as the unit.

The table shows that the time 1′ to 2 is longer than 2 to 1 in almost all cases. With the subject C. W., 1′ to 2 is always longer than 2 to 1′. With J. K., 1′ to 2 is longer than 2 to 1′ in all except one case out of 9. With M. M. again 1′ to 2 is longer than 2 to 1′ except in one case out of 10.

TABLE IX.

Drum beats.

Scheme : 1′–2.

Subject.	Average time from 1′ to 2.	Average time from 2 to 1′.	Number of measurements.	Ratio 1′ to 2 : 2 to 1′.
C. W.	681	658	10	1.00 : 0.97
	724	714	10	1.00 : 0.97
	750	736	10	1.00 : 0.98
	747	727	10	1.00 : 0.97
	774	753	10	1.00 : 0.97
	791	739	10	1.00 : 0.92
	788	755	10	1.00 : 0.96
	798	766	10	1.00 : 0.96
	782	744	7	1.00 : 0.95
J. K.	766	739	9	1.00 : 0.96
	659	620	10	1.00 : 0.94
	705	724	10	1.00 : 0.99
	737	732	10	1.00 : 0.99
	746	741	10	1.00 : 0.93
	776	726	10	1.00 : 0.98
	777	762	10	1.00 : 0.98
	701	684	10	1.00 : 0.98
	724	707	10	1.00 : 1.03
M. M.	817	793	10	1.00 : 0.97
	866	810	10	1.00 : 0.93
	856	799	10	1.00 : 0.93
	868	819	10	1.00 : 0.94
	852	797	10	1.00 : 0.93
	740	703	10	1.00 : 0.95
	825	783	9	1.00 : 0.95
	670	657	10	1.00 : 0.98
	699	681	10	1.00 : 0.97
	660	664	10	1.00 : 1.01

Unit of measurement, $\sigma = 0.001^s$.

Table X gives the results of the experiments with the scheme 1–2′. In obtaining the ratio of the two intervals 2′ to 1 is regarded as the unit. It will be observed from the table that again the interval which follows the emphasized beat is longer than that which follows the unemphasized one. The average time from 1 to 2′ is always shorter than 2′ to 1.

If we compare the schemes 1′–2 and 1–2′ the comparative length of the time from 2′ to 1 of 1–′2 is longer than that from 1′ to 2 of 1′–2. The average ratios of the intervals are:

	1′–2	1–2′
	1′ to 2 : 2 to 1′	1 to 2′ : 2′ to 1
M. M.	1.00 : 0.93	0.90 : 1.00
J. K.	1.00 : 0.91	0.90 : 1.00

This result agrees with that of the beating on the noiseless key, and can be accounted for by attributing it to the same cause. Since the

TABLE X.

Drum beats.

Scheme : 1–2′.

Subject.	Average time from 1 to 2′.	Average time from 2′ to 1.	Number of measurements.	Ratio 1 to 2′ : 2′ to 1.
C. W.	510	550	10	0.93 : 1.00
	499	543	10	0.91 : 1.00
	541	574	10	0.94 : 1.00
	534	561	10	0.95 : 1.00
	550	599	10	0.92 : 1.00
	559	591	10	0.95 : 1.00
	576	618	10	0.93 : 1.00
	567	611	10	0.93 : 1.00
	470	538	10	0.87 : 1.00
	489	523	10	0.91 : 1.00
J. K.	618	640	10	0.96 : 1.00
	652	658	10	0.99 : 1.00
	661	680	10	0.97 : 1.00
	680	685	10	0.99 : 1.00
	676	685	10	0.98 : 1.00
	657	698	10	0.94 : 1.00
	727	735	8	0.99 : 1.00
	693	718	10	0.96 : 1.00
	635	761	9	0.83 : 1.00
	678	683	10	0.99 : 1.00
M. M.	572	636	10	0.89 : 1.00
	672	756	10	0.89 : 1.00
	632	654	10	0.96 : 1.00
	629	601	10	0.91 : 1.00
	633	667	10	0.94 : 1.00
	583	641	10	0.91 : 1.00
	627	693	10	0.90 : 1.00
	579	628	10	0.92 : 1.00
	572	632	10	0.92 : 1.00
	575	622	10	0.92 : 1.00

Unit of measurement, $\sigma = 0.001^s$.

time 2′ to 1 of the scheme 1–2′ is an interval which comes between the rhythmic group, it is made longer by the "pause" in addition to the influence of the emphasis.

Table XI shows the results of the experiments on the scheme 1′–2–3. The proportion is obtained by regarding 1′ to 2 as a unit.

It will be observed in the table that the time 1′ to 2, which follows the emphasis is in general longer than 2 to 3 and 3 to 1′. The time 1′ to 2 is constantly longer than 2 to 3, but in some cases it is shorter than 3 to 1′, owing to the lengthening of the latter by the "pause."

The results of the experiment on the scheme 1–2′–3 are given in the Table XII. The time 2′ to 3 is longer than 1 to 2′ with exception of a few cases. The time 2′ to 3 is in general shorter than 3 to 1. This fact indicates that there is a strong tendency to lengthen the time be-

Table XI.

Drum beats.

Scheme : $1'$–2–3.

Subjects.	Average time from $1'$ to 2.	Average time from 2 to 3.	Average time from 3 to $1'$.	Number of measurements.	Ratios $1'$ to 2 : 2 to 3 : 3 to $1'$.
C. W.	611	604	597	10	1.00 : 0.99 : 0.97
	604	599	593	10	1.00 : 0.99 : 0.98
	582	592	588	10	1.00 : 1.02 : 1.01
	655	644	620	10	1.00 : 0.98 : 0.94
	628	618	618	10	1.00 : 0.98 : 0.98
	613	610	601	10	1.00 : 0.99 : 0.98
	669	647	646	10	1.00 : 0.96 : 0.96
	671	667	654	10	1.00 : 0.93 : 0.82
	674	668	650	10	1.00 : 0.99 : 0.96
	676	626	642	10	1.00 : 0.93 : 0.95
	656	643	629	9	1.00 : 0.98 : 0.95
	640	643	626	10	1.00 : 1.00 : 0.97
J. K.	414	392	400	10	1.00 : 0.94 : 0.97
	410	414	416	10	1.00 : 1.01 : 1.01
	432	406	404	10	1.00 : 0.94 : 0.94
	402	381	379	7	1.00 : 0.95 : 0.94
	438	419	421	10	1.00 : 0.95 : 0.96
	437	409	421	10	1.00 : 0.93 : 0.96
	439	415	427	10	1.00 : 0.94 : 0.97
	435	420	422	10	1.00 : 0.96 : 0.97
	428	415	427	10	1.00 : 0.97 : 0.99
	689	691	678	10	1.00 : 1.00 : 0.98
	685	677	670	10	1.00 : 0.98 : 0.97
M. M.	993	603	1063	8	1.00 : 0.63 : 1.07
	959	5[illegible]	1029	9	1.00 : 0.59 : 1.07
	1108	588	1034	9	1.00 : 0.53 : 0.93
	811	775	78[illegible]	7	1.00 : 0.95 : 0.93
	783	760	805	10	1.00 : 0.97 : 1.02
	795	776	807	10	1.00 : 1.97 : 1.01
	703	696	728	10	1.00 : 0.99 : 1.04
	748	694	737	10	1.00 : 0.93 : 0.98
	894	596	871	9	1.00 : 0.66 : 0.87

Unit of measurement, $\sigma = 0.001^s$.

tween the rhythmic groups. The same tendency is more marked here in 1–$2'$–3 than in $1'$–2–3.

The above observations will indicate how closely the results of the drum beats coincide with those of the beating on the noiseless key, notwithstanding the difference of the conditions in the two experiments, the one being a finger movement without sound, the other an arm movement with sound.

From the above two series of experiments the following general conclusions can be drawn :

1. The interval which follows an emphasized beat is lengthened.
2. The interval which comes between rhythmic groups is lengthened.

3. The lengthening of the interval between rhythmic groups is not equally great in all the rhythmic schemes.

TABLE XII.

Drum beats.

Scheme: 1–2′–3.

Subject.	Average time from 1 to 2′.	Average time from 2′ to 3	Average time from 3 to 1.	Number of measurements.	Ratios 1 to 2′ : 2′ to 3 : 3 to 1.
C. W.	551	540	540	10	1.00 : 1.00 : 0.99
	537	543	539	10	0.98 : 1.00 : 0.99
	534	530	540	10	1.01 : 1.00 : 1.02
	506	530	530	10	0.95 : 1.00 : 1.00
	574	597	598	10	0.96 : 1.00 : 0.98
	594	594	578	10	1.00 : 1.00 : 1.02
	550	542	563	10	0.97 : 1.00 : 1.01
	560	554	540	9	1.01 : 1.00 : 0.97
J. K.	444	453	463	10	0.97 : 1.00 : 1.02
	437	437	454	10	1.00 : 1.00 : 1.04
	433	435	435	10	0.99 : 1.00 : 1.00
	451	442	453	10	1.02 : 1.00 : 1.02
	406	425	437	10	0.95 : 1.00 : 1.03
	396	413	433	10	0.95 : 1.00 : 1.05
	411	422	439	10	0.97 : 1.00 : 1.04
	418	423	473	10	0.98 : 1.00 : 1.11
	460	475	472	10	0.97 : 1.00 : 0.99
	431	442	455	10	0.97 : 1.00 : 1.03
M. M.	790	867	899	7	0.91 : 1.00 : 1.04
	798	866	856	9	0.92 : 1.00 : 0.98
	823	908	928	9	0.96 : 1.00 : 1.02
	837	930	946	9	0.87 : 1.00 : 1.02
	809	886	894	10	0.91 : 1.00 : 1.01
	837	952	960	9	0.88 : 1.00 : 1.01
	881	945	975	10	0.93 : 1.00 : 1.03
	914	973	990	9	0.94 : 1.00 : 1.02
	890	970	983	10	0.91 : 1.00 : 1.01

Unit of measurement, $\sigma = 0.001^s$.

IV. INTENSITY AND TIME IN RHYTHM OF SPEECH.

We have already seen that emphasis tends to lengthen the interval in the rhythmic movements of finger and arm. Does the same thing hold true in the rhythm of speech?

In the rhythm of speech we must consider two things: (1) the relation of intensity to the time from the beginning of one syllable to the beginning of the next, and (2) the relation of intensity to the time actually occupied by the sound of the syllable.

GUEST[1] recognized the fact that there is a close relation between the accent and the length of a syllable. He says: "Besides the increase of

[1] GUEST, A History of English Rhythm, I 77, London 1838.

loudness, and the sharper tone which distinguish the accented syllable, there is also a tendency to dwell upon, or in other words, to lengthen its quantity. We cannot increase the loudness or the sharpness of a tone without a certain degree of muscular action, and to put the muscles in motion requires time."

BRÜCKE[1] recorded, with a marker on a smoked drum, the movements of a finger in beating time while he recited verses in iambic hexameter, alcaic and sapphic, in a scanning manner. It was found that the distances of the successive beats were equal.

KRÁL and MAREŠ[2] used the frog muscle preparation for recording the spoken sounds in Bohemian verse. A telephone was connected by wires to the motor nerve of the muscle, so that the vibrations of the diaphragm of the former interrupting the current would cause the muscle to contract. A pointer attached to the muscle registered the curves on a smoked drum. The results indicated that even with the same person the same vowel has a different length according as the emphasis is greater or less when a verse is recited in a scanning fashion: and that neither in intensity-verse nor time-verse are the lengths of feet ever exactly equal, the ratio of the emphasized half to the unemphasized half of a foot not keeping a relation like 1 : 1, but rather like 30 : 31, or 32 : 33.

In HURST and MCKAY'S[3] experiments on the time relation of poetical meters the subject recited poems representing each of the four usual meters, iambus, trochee, dactyl and anapest, while he beat in unison with the finger on a pointer which registered the length of the beats on a smoked drum. Experiments were also made on "pure" meters without words being thought or said. It was found that "an iambus consists of a short syllable followed by a long: a trochee of a long followed by a short, a dactyl of a long followed by two short syllables, and an anapest of two shorts followed by a long, yet with no fixed proportions between the syllables." In iambic meter the syllables have a ratio of about 1 : 2 and in the trochaic of 1 to a little less than 1.5 In anapestic meter the ratios were about 1 : 1 : 1.2 and in dactylic 1.6 : 1.1 : 1. In these experiments the investigators did not take any records of the spoken sounds, but only of the rhythmic strokes of the hand. The periods of the strokes of hand, however, are not identical with the lengths of the syllables.

[1] BRUCKE, Die physiologischen Grundlagen der neuhochdeutschen Verskunst, 23, Wien 1871.

[2] KRÁL A MAREŠ, *Trvání hlásek a slabi? dle objektivné miry*, Listy Filologické, 1893 IV 17.

[3] HURST AND MCKAY, *Experiments on the time relation of poetical meters*, Univ. of Toronto Stud., Psychol. Series, No. 3, 1899.

In the following experiments I have made an attempt to study the problem by taking records of spoken sounds.

For recording the speech vibrations an electric voice key[1] of a special form[2] was used (Fig. 5). The cylinder of hard rubber, 5^{cm} in length and

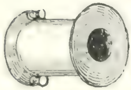

FIG. 5.

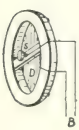

FIG. 6.

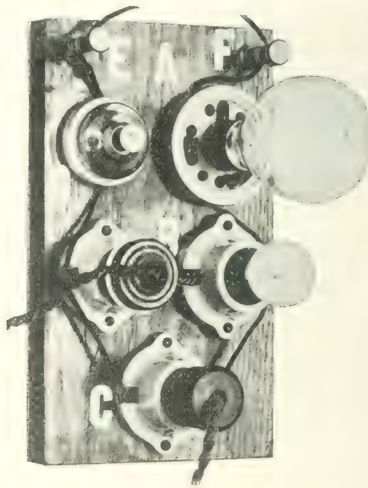

FIG. 7.

3^{cm} in diameter, is closed at one end by a thin sheet of platinum (*D* Fig. 6), while the other end is attached to mouthpiece. A

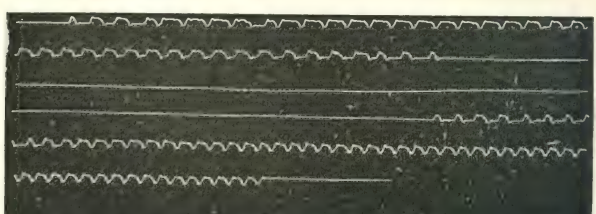

FIG. 8.

[1] CATTELL, *Psychometrische Untersuchungen*, Philos. Stud., 1886 III 313.
[2] SCRIPTURE, *Some new apparatus*, Stud. Yale Psych. Lab., 1895 III 107.

screw (S) with a platinum point is supported by a small metal rod: the point can be placed near the center of the sheet of platinum, so that the latter in its vibration will come in contact with the former and close an electric circuit.

The voice key was connected to one of the sockets of a four-socket lamp battery and the DEPREZ marker to the other, so arranged that the former made a high tension shunt around the latter.[1] The battery arrangement is indicated in Fig. 7; A indicates the intensity lamp, B the tension lamp, C the socket to which the key is connected, and G the socket to which the marker is connected the magnet. The marker recorded the results on the smoked surface of a drum which was run by a storage battery. The current of the battery being very constant the drum revolved at a uniform speed. The time line was drawn by the 100 v. d. fork.

With this method the cord vibrations in vowels can be recorded. A specimen record is given in Fig. 8; the time-line is at the bottom.

TABLE XIII.

Rhythm of speech.

Scheme: 1′–2.

Subject.	Average time from 1′ to 2.	Average time from 2 to 1′.	Number of measurements.	Ratio 1′ to 2 : 2 to 1′.
E	682	661	10	1.00 : 0.98
	689	690	10	1.00 : 1.00
	615	603	10	1.00 : 0.94
	696	673	10	1.00 : 0.94
K	658	663	10	1.00 : 1.01
	599	596	10	1.00 : 1.00
	943	935	10	1.00 : 0.97
B	765	743	10	1.00 : 0.97
	733	714	10	1.00 : 0.97

Unit of measurement, $\sigma = 0.001^s$.

TABLE XIV.

Rhythm of speech.

Scheme: 1–2′.

Subject.	Average time from 1 to 2′.	Average time from 2′ to 1.	Number of measurements.	Ratio 1 to 2′ : 2′ to 1.
E	691	846	10	0.81 : 1.00
	664	913	10	0.71 : 1.00
K	575	649	9	0.90 : 1.00
	563	936	10	0.89 : 1.00
B	575	774	10	0.74 : 1.00
	618	714	10	0.86 : 1.00

Unit of measurement, $\sigma = 0.001^s$.

[1] SCRIPTURE, *New apparatus and methods*, Stud. Yale Psych. Lab., 1896 IV 79.

Table XV.

Rhythm of speech.

Scheme : 1′–2–3.

Subject.	Average time from 1′ to 2.	Average time from 2 to 3.	Average time from 3 to 1′.	Number of measurements.	Ratios 1′ to 2 : 2 to 3 : 3 to 1′.
E	669	605	617	10	1.00 : 0.90 : 0.92
	630	594	613	10	1.00 : 0.94 : 0.97
K	709	615	606	10	1.00 : 0.86 : 0.85
	604	609	610	10	1.00 : 0.91 : 0.92

Unit of measurement, $\sigma = 0.001^s$.

Table XVI.

Rhythm of speech.

Scheme : 1–2′–3.

Subject.	Average time from 1 to 2′.	Average time from 2′ to 3.	Average time from 3 to 1.	Number of measurements.	Ratios 1 to 2′ : 2′ to 3 : 3 to 1.
E	590	600	614	10	0.98 : 1.00 : 1.02
	616	622	633	10	0.97 : 1.00 : 1.02
K	713	751	775	10	0.95 : 1.00 : 1.03
	697	722	786	10	0.96 : 1.00 : 1.09
	717	751	759	10	0.95 : 1.00 : 1.01

Unit of measurement, $\sigma = 0.001^s$.

Table XVII.

Rhythm of speech.

Scheme : 1′–2.

Subject.	Average length of a_1'.	Average length of a_2.	Number of measurements.	Ratio $a' : a$.
E	368	317	10	1.00 : 0.86
	392	355	10	1.00 : 0.93
	445	389	10	1.00 : 0.87
	417	355	10	1.00 : 0.83
K	325	311	10	1.00 : 0.96
	355	306	10	1.00 : 0.86
	787	373	10	1.00 : 0.96
B	451	273	10	1.00 : 0.61
	448	307	10	1.00 : 0.68

Unit of measurement, $\sigma = 0.001^s$.

During the experiments the subject was put in the silent room as in the preceding experiments. He held the voice key in his left hand and recited a series of *a* sounds (like *a* in *father*) in a scanning manner, changing the loudness according to different rhythmic schemes.

Tables XIII to XVI give the results of the measurements of the lengths of the intervals. For the interval, the distance between the be-

TABLE XVIII.

Rhythm of speech.

Scheme : 1–2′.

Subject.	Average length of a_1.	Average length of a_2'.	Number of measurements.	Ratio $a : a'$.
E	339	368	10	0.92 : 1.00
	322	381	10	0.87 : 1.00
K	294	345	10	0.85 : 1.00
	310	350	10	0.90 : 1.00
B	275	395	9	0.69 : 1.00
	322	400	10	0.80 : 1.00

Unit of measurement, $\sigma = 0.001^s$.

TABLE XIX.

Rhythm of speech.

Scheme : 1′–2–3.

Subject.	Average length of a_1'.	Average length of a_2.	Average length of a_3.	Number of measurements.	Ratios $a' : a : a$.
E	342	280	277	10	1.00 : 0.82 : 0.81
	330	288	256	10	1.00 : 0.87 : 0.77
K	312	240	195	10	1.00 : 0.76 : 0.63
	322	294	242	10	1.00 : 0.89 : 0.78

Unit of measurement, $\sigma = 0.001^s$.

TABLE XX.

Rhythm of speech.

Scheme : 1–2′–3.

Subject.	Average length of a_1.	Average length of a_2'.	Average length of a_3.	Number of measurements.	Ratios $a : a' : a$.
E	308	371	283	10	0.83 : 1.00 : 0.79
	351	395	329	10	0.99 : 1.00 : 0.93
K	377	410	378	10	0.92 : 1.00 : 0.92
	396	439	406	10	0.90 : 1.00 : 0.92
	353	382	348	10	0.92 : 1.00 : 0.91

Unit of measurement, $\sigma = 0.001^s$.

ginning of a sound and the beginning of the next sound was measured. The unit of measurement was $\sigma = 0.001^s$. The ratios are found by regarding the interval which follows the emphasized beat as the unit in all the four different rhythmic schemes.

It will be seen from the tables that the results are substantially the same as those of the foregoing experiments in tapping with the finger and in

the drum beats: (1) the interval which follows the accented syllable is lengthened: (2) the interval between the rhythmic groups is lengthened.

In the scheme 1–2′, the time from 2 to 1′ is constantly longer than 1 to 2′, but in 1′–2, 1′ to 2 is in some cases shorter than 2 to 1′. This is due to the same fact noted before, that the interval between the rhythmic groups includes a pause. The same phenomenon is more marked in the scheme 1–2′–3 than in 1′–2–3.

Tables XVII, XVIII, XIX, XX give the lengths of the accented and unaccented syllables. In the ratios the average length of the accented syllable is always regarded as the unit. The unit of measurement of the average length is $\sigma = 0.001^s$.

The following facts can be seen in the tables: (1) the accented syllable is always longer than the unaccented syllable; (2) the last syllable of a rhythmic group is not lengthened unless it is accented. In this respect the length of a syllable differs from the length of an interval.

V. Intensity and pitch in rhythm of speech.

According to Mitford,[1] the strengthened syllables in English have an acuter tone or a higher note. The fact can be abundantly proved, he supposed, if we find or coin a word which is composed of syllables without variety of vowel sound and pronounce it with a strong accent on either syllable.

Müller[2] noticed that in a larynx separated from the body the pitch of the tone might be raised by an increase of the force of blast. He thought that one of the modes of producing high notes without increasing the tension of the vocal ligament is to blow with greater force, by which means the notes may without difficulty be raised through a series of semi-tones to the extent of a "fifth."

I found it possible to make some observations concerning the relation between intensity and pitch in the records which were taken in the preceding experiments. It must be remembered that the subjects of the experiments were requested to recite, in a scanning manner, a series of *a* sounds, changing the intensity according to the prescribed rhythmic scheme, no instruction being given as to the pitch of the tone.

The wave lines in the records corresponded to the periods of the cord vibrations (see Fig. 8). The first step in the study of the pitch in the

[1] Mitford, Inquiry into the Principle of Harmony in Language and of the Mechanism of Verse, Modern and Ancient, 57, London 1804.

[2] Müller, The Physiology of the Senses, Voice and Muscular Motion, with the Mental Faculty, trans. by Baily, London 1848.

records was to measure the actual lengths of the waves in succession. This gave the lengths of the successive periods in the cord vibrations.

The following are the results of the measurements. The unit for the lengths of the periods is $^{\sigma} = 0.001^{s}$.

A. Rhythmic scheme: 1′–2.

Subject K.

Example 1. The successive periods of a' were: 7, 7, 7, 7, 7, 6, 6, 6, 6, 6, 6, 6, 6, 5, 5, 5, 5, 5, 5, 5, 5, 5, 5, 5, 5, 5, 5, 5, 5, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4, 4^{σ}. The successive periods of a were: 8, 8, 8, 8 8, 8, 8, 8, 8, 8, 8, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7, 7^{σ}. Or more briefly, a': 7^{σ} (5 times), 6^{σ} (8 times), 5^{σ} (16 times), 4^{σ} (38 times); a: 8^{σ} (11 times), 7^{σ} (32 times).

The changes in the lengths of the periods were not sudden but very gradual in this record as well as in all other records. The accented syllable began with a period of 7^{σ} and changed gradually through 6^{σ} and 5^{σ} to 4^{σ}, which was reached at the 30th vibration and maintained to the end. In other words the pitch changed upward from 143 complete vibrations per second, through 170 and 200 to 250 per second.

The unaccented syllable, on the other hand, began with a period of 8^{σ}, and reached 7^{σ} at the 12th vibration, which was kept to the end. That is, the pitch changed from 125 upward to 143 complete vibrations a second.

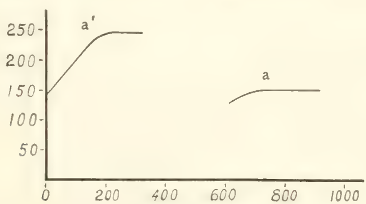

Fig. 9.

The results are shown in Fig. 9. In this, as well as in the other similar figures, the horizontal axis indicates time, while the vertical ordinate gives the number of cord vibrations a second. The space between the curves corresponds to the empty interval between the syllables.

Example 2. The a' had 22 vibrations, occupying the successive periods as follows: 8^{σ} (7 times), 7^{σ} (5 times), 6^{σ} (4 times), 5^{σ} (4 times).

The *a* consisted of 38 vibrations and the successive periods run: 9^σ (12 times), 8^σ (6 times), 7^σ (20 times).

The accented syllable began with 125 vibrations a second and changed upward through 143 and 167 to 200 vibrations a second. About three-fourths of the whole length were occupied by 200 vibrations a second,

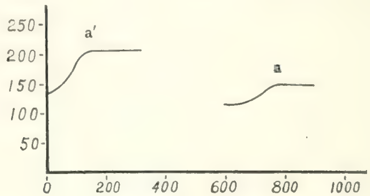

Fig. 10.

which extended from the 17th vibration to the end. The unaccented *a* began with 111 vibrations a second and changed through 125 to 143. The results are shown in Fig. 10.

Subject E.

Example 1. The successive periods of *a'* were: 7^σ (12 times), 6^σ (17 times), 5^σ (52 times); those of *a* were: 7^σ (43 times), 6^σ (21 times).

The pitch of the accented sound glided upward from 143 vibrations a second through 176, reaching 200 at the 30th vibration, which was kept to the end. The unaccented sound began with 143 vibrations a second and changed through 176. It must be noticed here that although

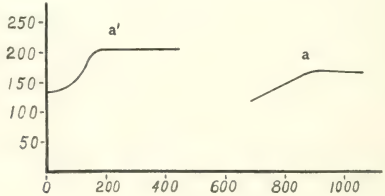

Fig. 11.

a' as well as *a* began with the same pitch, 143 vibrations a second, the former reached 167 at the 13th vibration, while the latter reached the same pitch not before the 44th vibration. The greater part of *a* was occupied by 120 vibrations per second, while about the two-thirds of *a'* were occupied by 143. The results are shown in Fig. 11.

Example 2. The successive periods of a' were: 7^σ (5 times), 6^σ (15 times), 5^σ (50 times); and those of a: 7^σ (18 times), 6^σ (39 times).

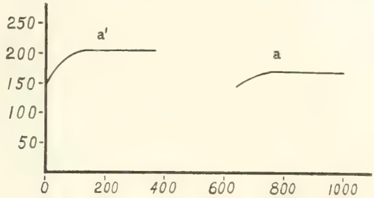

FIG. 12.

The pitch of a' beginning with 143 vibrations a second rose through 167 to 200; a began with 143 and changed slowly to 174. The results are shown in Fig. 12.

Subject B.

Example 1. The successive periods of a' were: 8^σ (14 times), 7^σ (58 times); those of a: 10^σ (40 times).

The pitch of a' began with 125 vibrations a second and rose gradually, reaching 143 at the 15th vibration. The pitch of a was constant at 100 vibrations a second. The results are given in Fig. 13.

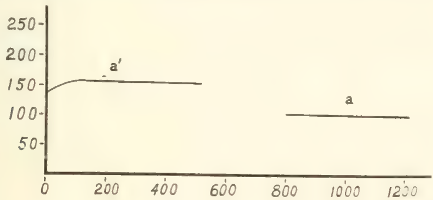

FIG. 13.

Example 2. The total number of vibrations of a' was 67, the successive periods occupying: 8^σ (25 times), 7^σ (42 times). The a had 26

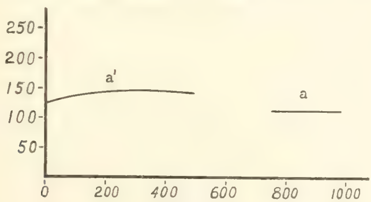

FIG. 14.

vibrations of a constant period of 9^σ. The pitch of a' changed from 125 vibrations a second upward to 167, while that of a was uniform at 111, as indicated in Fig. 14.

B. Rhythmic scheme: 1–2′.

Subject K.

Example 1. The successive periods of a were: 9^σ (19 times), 8^σ (22 times); those of a' were: 8^σ (7 times), 7^σ (4 times), 6^σ (5 times), 5^σ (56 times).

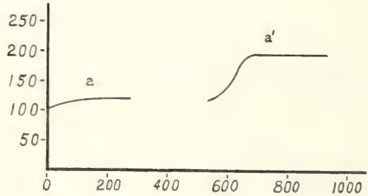

Fig. 15.

The pitch of a beginning with 111 gradually rose to 125 vibrations a second; a' began with 125 vibrations a second and glided upward through 143 and 167 to 250, as indicated in Fig. 15.

Example 2. The successive periods of a were: 9^σ (8 times), 8^σ (32 times); of a': 8^σ (9 times), 7^σ (4 times), 6^σ (9 times), 5^σ (46 times). The pitch of the emphasized a changed from 111 vibrations a second

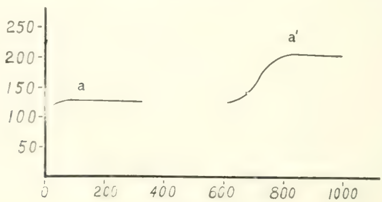

Fig. 16.

to 125, while that of the unemphasized a began with 125 vibrations a second and underwent gradual change through 143 and 167 to 200, as indicated in Fig. 16.

The change of the rhythmic scheme does not affect the pitch. The accented syllable has a higher pitch than the unaccented syllable as before.

Subject B.

Example 1. The successive periods of a were: 10^σ (30 times); those of a': 7^σ (53 times), 6^σ (4 times). The pitch of a was constant

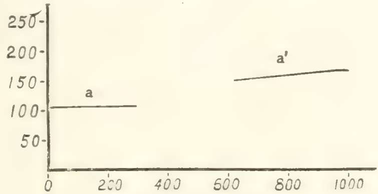

FIG. 17.

at 100 vibrations a second; the pitch of a' on the contrary began with 143 vibrations a second and rose toward the end to 167, as indicated in Fig. 17.

Example 2. The successive periods of a were: 9^σ (32 times), those of a': 8^σ (3 times), 7^σ (51 times). The pitch of the unaccented vowel

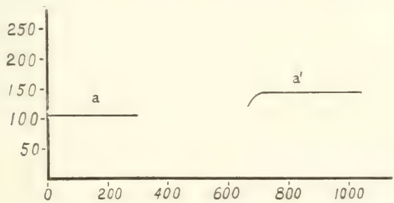

FIG. 18.

was again lower and more uniform than that of the accented. The latter beginning with 125 vibrations a second reached 143 at the 4th vibration, which was kept to the end as indicated in Fig. 18.

C. *Rhythmic scheme:* 1′–2–3.

Subject K.

Example 1. The successive periods of a' were: 7^σ (4 times), 6^σ (5 times), 5^σ (8 times), 4^σ (42 times); the middle a occupied the successive periods: 9^σ (3 times), 8^σ (29 times); the last a: 9^σ (25 times). The pitch of the accented syllable beginning with 143 vibrations a second rose upward through 167 and 200, reaching 250 at the 18th vibration. The pitch of the first unaccented a began with 111 vibrations a second

and rose after three vibrations to 125, which was maintained to the end. The second unaccented a was constant at 111 vibrations a second. The results are indicated in Fig. 19.

Example 2. The successive periods of a' were: 7^σ (10 times), 6^σ (11 times), 5^σ (7 times), 4^σ (32 times); the middle a: 9^σ (9 times), 8^σ

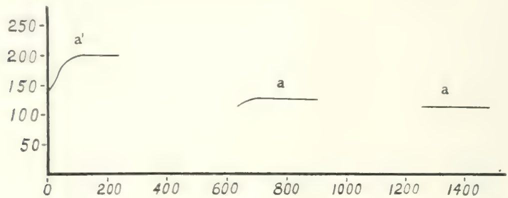

FIG. 19.

(23 times); and those of the last a: 8^σ (31 times). The mode of change of pitch is substantially the same as the preceding example. The accented syllable beginning with 143 vibrations a second rose through 167 and 200 to 250, which was reached at the 28th vibration.

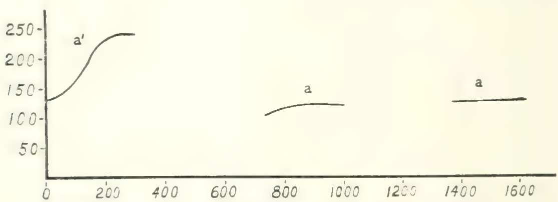

FIG. 20.

The first unaccented syllable began with 111 vibrations a second and changed to 125. The second unaccented syllable was constant at 125 vibrations a second and is uniform. The results are indicated in Fig. 20.

Subject E.

Example 1. The successive periods of a' were: 6^σ (22 times), 5^σ (37 times); those of the middle a: 8^σ (37 times), and those of the last a: 8^σ(24 times), 9^σ (4 times), 10^σ (4 times). The pitch of the accented syllable beginning with 167 vibrations a second rose to 200. The first unaccented syllable was constant at 125 vibrations a second. The pitch of the second unaccented syllable began with 125 vibrations a second

and glided downward through 111 to 100. The results are indicated in Fig. 21.

Example 2. The successive periods of a' were: 7^{σ} (11 times), 6^{σ} (8 times), 5^{σ} (41 times); those of the middle a: 8^{σ} (40 times); and those

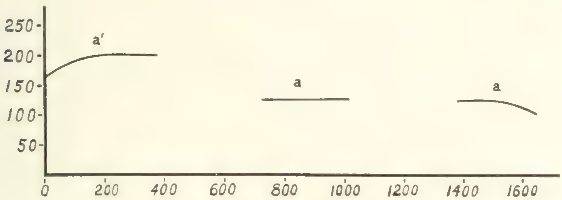

FIG. 21.

of the last a: 8^{σ} (23 times), 9^{σ} (4 times), 10^{σ} (9 times). The pitch of a' begins with 143 vibrations a second and rose through 167 to 200 which, being reached at the 20th vibration, was kept to the end. The pitch of the first unaccented a was constant at 125 vibrations a second.

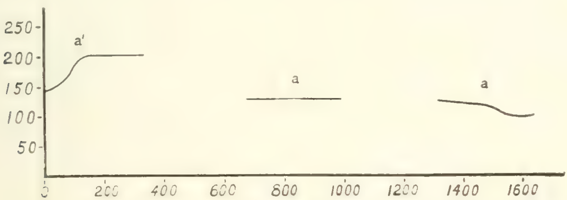

FIG. 22.

That of the second unaccented a began with 125 and glided downward through 111 to 100. The results are indicated in Fig. 22.

We have to observe here that there are some individual differences between the two subjects in regard to the mode of the change of the pitch. With the subject K, the first unaccented syllable is constant, while with E, the second unaccented syllable is constant. With K, the change of pitch is always from lower to higher, but with E the second unaccented syllable in this scheme goes from higher to lower.

D. Rhythmic scheme: 1–2′–3.

Subject K.

Example 1. The successive periods of the first a were: 8^{σ} (9 times), 7^{σ} (48 times); those of a': 7 (2 times), 6^{σ} (6 times), 5^{σ} (9 times), 4^{σ}

86 times); those of the last *a* : 9$^\sigma$ (12 times), 8$^\sigma$ (18 times), 7$^\sigma$ (9 times). The pitch of the first unaccented syllable beginning with 125 vibrations a second changed to 143. The unaccented syllable began with 143 vibrations a second and rose through 167 and 200 up to 250

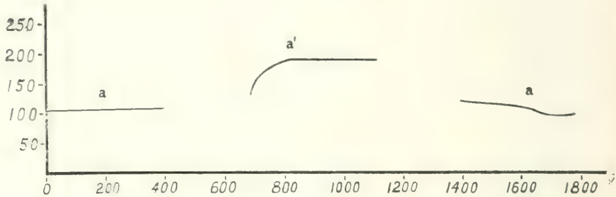

FIG. 23.

at the 18th vibration. That of last *a* began with 111 vibrations a second and glided upward through 125 to 143. The results are shown in Fig. 23.

Example 2. The successive periods of the first *a* were : 9$^\sigma$ (9 times), 8$^\sigma$ (7 times), 7$^\sigma$ (17 times), 6$^\sigma$ (24 times). Those of *a'* were : 8$^\sigma$ (3 times), 7$^\sigma$ (4 times), 6$^\sigma$ (5 times), 5$^\sigma$ (12 times), 4$^\sigma$ (74 times); those of the second *a* : 9$^\sigma$ (9 times), 8$^\sigma$ (6 times), 7$^\sigma$ (11 times), 6$^\sigma$ (33 times). The first unaccented *a* began with 111 and glided upward

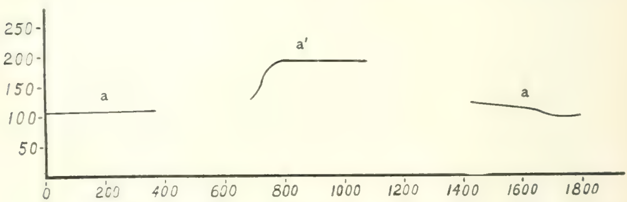

FIG. 24.

through 125 and 143 to 167. The emphasized *a'* beginning with 125 changed upward through 143, 167 and 200 to 250 at the 24th vibration, from which point the pitch was constant. The last *a* began with 111 and changed through 125 and 143 to 167. The results are shown in Fig. 24.

Subject E.

Example 1. The period of the first *a* was constant at 8$^\sigma$ (49 times). The periods of *a'* were : 7$^\sigma$ (2 times), 6$^\sigma$ (13 times), 5$^\sigma$ (66 times) ; those of the last *a* : 8$^\sigma$ (27 times), 9$^\sigma$ (10 times), 10$^\sigma$ (7 times). The

pitch of first *a* was constant at 125 vibrations a second; the *a'* beginning with 143 vibrations a second changed through 167 to 200, which occu-

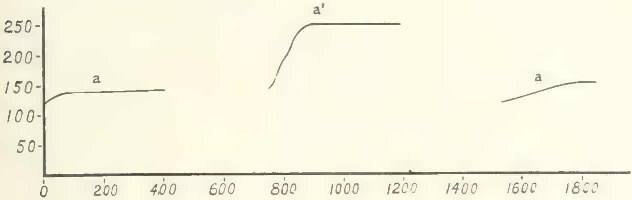

FIG. 25.

pied the most part of the entire length; the last *a* began with 125 vibrations a second and glided downward through 111 to 100 (Fig. 25).

Example 2. The period of the first *a* was constant at 8^σ (46 times). The periods of the *a'* were: 7^σ (5 times), 6^σ (5 times), 5^σ (66 times); those of the last *a*: 8^σ (28 times), 9^σ (6 times), 10^σ (10 times). The pitch of the first *a* was constant at 125 vibrations a second, as in the

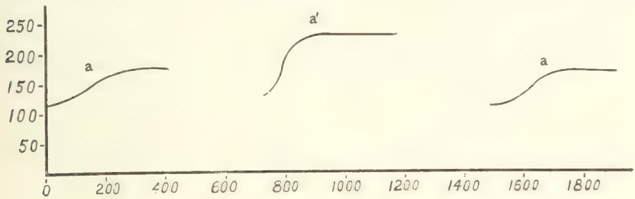

FIG. 26.

last example, that of *a'* beginning with 143 changed upward through 167 to 200. The last *a* beginning with 125 vibrations a second glided downward through 111 to 100 (Fig. 26).

We observe again that there are individual differences between the two subjects in the mode of the change of the pitch. With the subject K the first *a* underwent a change, while with E it was constant; with K, the last *a* glided upward, while with E it glided downward.

E. Conclusions.

From the above observations the following conclusions can be drawn:

1. The accented syllable has a higher pitch than the unaccented syllable.

2. The accented syllable begins in general with a higher pitch than the unaccented syllable.

3. Even in the cases where both accented and unaccented syllables begin with the same pitch, the former glides upward higher than the latter.

4. The pitch of the accented syllable undergoes greater changes than that of the unaccented one.

5. The pitch of the accented syllable always glides upward.

6. The pitch of the unaccented syllable also glides upward in the majority of cases, but sometimes glides downward.

SWEET[1] accounts for the relation between intensity and pitch by attributing it to an emotional attitude of the subject. He thinks "all energetic emotions naturally express themselves in high tones and forceful utterance, and increased vehemence of emotion is accompanied by a rise in force and pitch." This explanation is hardly applicable to our case, because we cannot suppose that in reciting a series of simple sounds, like *a*, a change of emotion would take place that would bring about such a difference between the accented and unaccented syllables.

MITFORD[2] supposed that when we pronounce an accented syllable, we raise the tongue near to the palate, with the consequence of the rise of the height of tone. "To produce the proper English intonation" he says "the tongue must be raised up in pronouncing the strengthened syllable, the vibration will be felt more about the palate and the tone will be acuter, it will be a higher note." The change of the position of tongue in the mouth cavity would only affect the resonance tone and not the cord vibration. It thus gives no explanation of the fact.

It seems that the more probable explanation must be sought in the nature of the action of the larynx. BRÜCKE[3] supposed that in strong accentuation the vocal cords on account of the strong pressure of the air are more stretched and come closer to each other and that, as a consequence of the increase of the tension of the cords, the pitch of the tone is raised. SCRIPTURE[4] thinks that the relation between the rise of pitch of the cord tone and the increase in the force of the puff would naturally result from a gradual tightening of the vocal muscles which is due to associated habits of innervation and not to the physical effect of the air pressure in stretching the cords.

The phenomenon perhaps depends also on the nature of the rhythm of speech itself. The three elements of rhythm: intensity, length and pitch, all have the function of producing the emphasis and, being closely associated in our minds, would naturally tend to occur together. This does not mean of course that a rise in pitch must necessarily occur with an

[1] SWEET, A Primer of Phonetics, 67, Oxford 1890.

[2] MITFORD, An Inquiry into the Principles of Harmony in Language and of the Mechanism of Verse, Modern and Ancient, 62, London 1804.

[3] BRÜCKE, Die physiologischen Grundlagen der neuhochdeutschen Verskunst, 3, Wien 1871.

[4] SCRIPTURE, *Nature of vowels*, Amer. Journ. Sci., 1901 XI 302.

increase of intensity in the rhythm of speech. The two elements can be separated according to different mental conditions. It can be avoided by voluntary control or as the result of practice. MÜLLER[1] says that "since the human organ of voice possesses the power of increasing the intensity of a note from the faintest 'piano' to 'fortissimo,' without its pitch being altered, there must be some other means of compensating the tendency of the vocal cords to emit a higher note when the force of the current of the air is increased. This means evidently consists in modifying the tension of the vocal cords. When a note is rendered more intense, the vocal cords must be relaxed by remission of the muscular action in proportion as the force of the current of the breath through the glottis is increased. When the note is rendered fainter, the reverse of this must occur."

VI. BEATING TIME IN CONNECTION WITH RHYTHM OF SPEECH.

The following experiments were made to determine where the moment of the beat of the finger comes when it beats time in connection with the rhythm of speech.

MEYER,[2] working on the same problem with the purpose of determining the position of the arsis in rhythmic articulation, used for recording the voice a mouth trumpet, ending in a MAREY tambour covered with a fine rubber membrane to which a small straw lever ending in a light pointer was attached. The beat of the finger was made on an apparatus composed of plates of hard rubber connected by a string to a time marker. The subject recited some syllables into the tambour through the trumpet, while he beat time on the rubber plate. Thus the breath curve and the moment of beating could be recorded simultaneously on the smoked drum. In all cases, except where the syllable began with a voiced explosive (*b*, *d*, *g*), the beat came before the vowel. Both the tambour and the beating apparatus, used in the experiment, had considerable latent times which could be only roughly measured to be about 8^σ for the former and 25^σ for the latter. A somewhat more accurate method seemed desirable.

In the following experiments I used the voice key described above on p. 24. As the light diaphragm of platinum vibrated very easily at a short distance from the mouth, it recorded the first vibration of the voice with a latent time of not over half a thousandth of a second. The voice key was put in one of the wire circuits of a lamp battery and a DEPREZ marker in the other, as in the preceding experiments (p. 25). The

[1] MÜLLER, The Physiology of the Senses, Voice and Muscular Motion, with the Mental Faculties, trans. by Baily, 1034, London 1848.

[2] MEYER, *Beiträge zur deutschen Metrik*, Neuere Sprachen, 1898 VI 1,121.

latent time of the marker was less than 1$^{\sigma}$ as had been previously determined by frequent tests.

For the beating apparatus the noiseless key in the rubber bag (p. 8) was used. The tension of the key was very small and the slightest touch was enough to overcome the resistance for breaking the contract; the time lost in compression of the finger before the key acted was infinitesimal. The key was connected to the primary circuit of a spark coil while the metallic point of the DEPREZ marker was attached to one pole of the secondary circuit. The arrangement for drawing the time line was the usual one of a 100 v. d. fork. The drum was run by a motor with the storage battery; a very constant speed was attained.

The subject held the voice key in his hand and, putting its mouthpiece close to his lips, recited a syllable in a scanning manner, while he beat time on the noiseless key with the finger of his right hand (generally the index finger), the rate of the movement being left to his choice.

The following syllables were used by different subjects: (1) *a*, (2) *'a*, (3) *ma*, (4) *ha*, (5) *pa*, (6) *ăp*, (7) *āp*, (8) *măm*, (9) *mām*. In these the *a* was pronounced like *a* in "father." The *'a* was the same as *a*, but with slight glottal catch at the beginning. Both *ă* and *ā* were the same as *a*, but *ă* was shorter than *ā*, as the sign indicates. All the consonants were pronounced as in English words.

The subjects were four: K, E, T and M. Ten records were generally taken at each single experiment. The results of the experiments are given in the Tables XXI to XXIX. In all the tables the positive signs indicate the deviations when the beats of the finger came before the vowel, and the negative ones those when the beats came after the beginning of the vowel. The first column gives the initials of the subjects, the

TABLE XXI.

Finger beat with 'a.'

Subject.	Time of beat before *a*.	Number of measurements.	Number of —.	Number of —.	Immediate probable error.
K.	− 22	10	9	1	10.2
	− 8	10	6	4	13.5
	+ 40	10	10	0	23.0
	− 10	10	7	3	13.0
	− 21	10	8	1	13.4
	− 11	10	8	2	14.6
T.	− 61	10	9	1	32.5
	+ 80	10	10	0	34.5
	− 108	10	10	0	38.6
	− 38	10	10	0	20.7
	− 75	10	10	0	27.0
	− 68	10	10	0	20.1

Unit of measurement, $\sigma = 0.001^s$.

second the average deviation of the beats of the finger from the beginning of the vowel. The unit of the measurement is $\sigma = 0.001^s$. The third column gives the number of records from which the average is obtained. The fourth and fifth columns give the number of the cases in which the positive and negative deviations occurred respectively. The probable errors are calculated according to the formula used above (p. 6). A summary of the results of the experiments is given in Tables XXX and XXXI. Table XXX contains the average results of individual subjects for different syllables and Table XXXI the total averages from all the results.

TABLE XXII.

Finger beat with ''a.'

Subject.	Time of beat before *a*.	Number of measurements.	Number of +.	Number of —.	Immediate probable error.
E	+ 1[illegible]	10	10	0	15.7
	+ 13[illegible]	10	10	0	22.0
	+ 112	10	10	0	23.0
	+ 181	10	10	0	10.2
	+ 121	10	10	0	19.2
M	+ 177	10	10	0	12.7
	+ 147	10	10	0	10.5
	+ 80	10	10	0	22.5
	+ 117	10	10	0	40.6

Unit of measurement, $\sigma = 0.001^s$.

TABLE XXIII.

Finger beat with 'ma.'

Subject.	Time of beat before *a*.	Number of measurements.	Number of +.	Number of —.	Immediate probable error.
K	+ 112	10	10	0	28.0
	+ 60	10	9	1	22.2
	+ 70	10	10	0	12.3
	+ 70	10	10	0	12.0
	+ 74	10	10	0	23.0
	+ 70	10	10	0	14.4
E	+ 140	10	10	0	16.0
	+ 124	10	10	0	13.3
	+ 104	10	10	0	14.9
	+ 123	10	10	0	17.2
	+ 111	10	10	0	11.2
T	+ 245	10	10	0	
	+ 129	10	10	0	39.8
	+ 208	10	10	0	27.6
	+ 83	10	10	0	39.0
	+ 261	10	10	0	30.3
	+ 80	10	10	0	41.1
M	+ 177	10	10	0	8.8
	+ 234	10	10	0	19.5
	+ 118	10	10	0	8.4
	+ 123	10	10	10	33.1

Unit of measurement, $\sigma = 0.001^s$.

Table XXIV.

Finger beat with 'ha.'

Subject.	Time of beat before *a*.	Number of measurements.	Number of +.	Number of —.	Immediate probable error.
K.	+ 38	10	10	0	18.4
	+ 27	10	9	1	13.1
	+ 93	10	10	0	13.5
	+ 77	10	10	0	8.8
	+ 59	10	10	0	21.3
	+ 49	10	10	0	20.7
E.	+ 110	10	10	0	12.0
	+ 84	10	10	0	14.7
	+ 87	10	10	0	15.0
	+ 78	10	10	0	25.1
	+ 159	10	10	0	10.2
T.	+ 105	10	10	0	46.8
	+ 134	10	10	0	20.1
	+ 106	10	10	0	24.6
	+ 44	10	8	1	20.7
	+ 111	10	10	0	20.1
	+ 70	10	10	0	19.6
M.	+ 174	10	10	0	15.6
	+ 182	10	10	0	19.7

Unit of measurement, $\sigma = 0.001^s$.

Table XXV.

Finger beat with 'pa.'

Subject.	Time of beat before *a*.	Number of measurements.	Number of +.	Number of —.	Immediate probable error.
K	+ 59	10	9	1	26.6
	+ 75	10	10	0	17.9
	+ 108	10	10	0	14.6
	+ 62	10	10	0	16.7
	+ 70	10	10	0	21.0
	+ 83	10	10	0	18.5
E	+ 149	10	10	0	21.3
	+ 85	10	10	0	9.3
	+ 142	10	10	0	21.3
	+ 118	10	10	0	13.6
	+ 119	10	10	0	10.2
I	+ 287	8	8	0	
	+ 207	10	10	0	24.5
	+ 170	8	8	0	61.0
	+ 135	10	10	0	15.4
	+ 152	10	10	0	21.3
	+ 161	10	10	0	45.8
M	+ 209	10	10	0	18.8
	+ 149	10	10	0	14.7
	+ 120	10	10	0	14.7
	+ 157	10	10	0	12.6

Unit of measurement, $\sigma = 0.001^s$.

Table XXVI.

Finger beat with 'ăp.'

Subject.	Time of beat before *a*.	Number of measurements.	Number of +.	Number of —.	Immediate probable error.
K	+ 51	10	10	0	11.2
	+ 35	10	10	0	4.7
	+ 43	10	9	1	6.8
	+ 11	10	6	4	10.4
	+ 31	10	10	0	8.1
	+ 11	10	7	3	8.6
E	+ 89	10	10	0	7.9
	+ 126	10	10	0	14.0
	+ 54	10	10	0	9.8
	+ 84	10	10	0	30.1

Unit of measurement, $\sigma = 0.001^s$.

Table XXVII.

Finger beat with 'āp.'

Subject.	Time of beat before *a*.	Number of measurements.	Number of +.	Number of —.	Immediate probable error.
K	+ 13	10	7	3	6.4
	+ 43	10	9	1	11.6
	— 23	10	8	2	14.2
	+ 4	10	5	5	17.4
	— 3	10	4	6	12.6
	— 19	10	2	8	15.9
E	+ 99	10	10	0	20.1
	+ 94	10	10	0	20.2
	+ 90	10	10	0	22.5

Unit of measurement, $\sigma = 0.001^s$

Table XXVIII.

Finger beat with 'măm.'

Subject.	Time of beat before *a*.	Number of measurements.	Number of +.	Number of —.	Immediate probable error.
K	+ 70	10	10	0	18.6
	— 81	10	10	0	28.8
	— 32	10	10	0	13.1
	— 33	10	8	0	26.5
E	+ 60	10	10	0	10.0
	+ 60	10	10	0	12.3
	+ 53	10	10	0	22.1
	— 71	10	10	0	13.8

Unit of measurement, $\sigma = 0.001^s$.

Table XXIX.

Finger beat with 'mām.'

Subject.	Time of beat before *a*.	Number of measurements.	Number of +.	Number of —.	Immediate probable error.
E	+ 66	10	10	0	13.0
	+ 73	10	10	0	12.2
	+ 67	10	10	0	7.1
	+ 53	10	10	0	13.9
K	+ 59	10	9	0	24.7
	+ 65	10	10	0	28.8
	+ 51	10	9	1	27.8
	+ 72	10	10	0	24.4

Unit of measurement, $\sigma = 0.001^s$.

Table XXX.

Summary of Tables XXI to XXIX, for individuals.

Subject.	*ma*	*pa*	*ha*	*'a*	*a*	*ăp*	*āp*	*mām*	*mam*
K	+ 7[illegible]	+ [illegible]	+ 5[illegible]		+ 10	+ 30	+ 10	+ 54	+ 60
E	+ 120	+ 140	+ 103	+ 132		+ 88	+ 94	+ 61	+ 64
T	+ 100	+ 181	+ 133		+ 86				
M	+ 103	+ 157	+ 172	+ 130					

Unit of measurement, $\sigma = 0.001^s$.

Table XXXI.

Summary of Tables XXI to XXIX, for sounds.

	Average time of beat before vowel.	Number of measurements.	Number of +.	Number of —.
ma	+ 132	210	209	1
pa	+ 143	206	205	1
ha	+ 118	190	187	2
'a	+ 131	70	90	0
a	+ 52	120	107	12
ăp	+ 59	100	92	8
āp	+ 52	90	65	25
mam	+ 57	80	80	1
mām	+ 62	80	78	1

Unit of measurement, $\sigma = 0.001^s$.

The tables show that the beat of the finger comes before the beginning of the vowel in all the following conditions:

(1) when the vowel is preceded by a consonant and is not followed by any other sound;

(2) when the vowel has the glottal catch at the beginning;
(3) when the vowel is neither preceded nor followed by any sound;
(4) when a short vowel is followed by a consonant;
(5) when a long vowel is followed by a consonant;
(6) when the short vowel is preceded and followed by consonants;
(7) when the long vowel is preceded and followed by consonants.

It will be observed also that the amount of time by which the beat occurs before the beginning of the vowel is not the same in the different combinations in which the vowel stands.

The results for the subjects K and T show that the length of time by which the beat occurs before *a*, when not preceded by a consonant, is considerably shorter than that before the vowel when preceded by a consonant. This fact indicates that the consonant lengthens the time between the beat and the beginning of the vowel.

The amount of time between the beat and the beginning of the vowel differs with the different consonants which precede it. The subjects K, E, T all agree in making this difference greatest in *pa*, the next greatest in *ma*, and least in *ha*.

The amount of time by which the beat is ahead in *'a* is not very different from that in *ma*, *pa* and *ha*. It is probably due to the fact that the glottal catch at the beginning of the vowel is of the same nature as a consonant in so far as the complexity of action of the vocal organs is concerned.

The results for *măm* and *mām* seem to indicate, if not in a very conclusive manner, that when a vowel is preceded as well as followed by a consonant, the beat tends to come nearer the beginning of the vowel than when the vowel is preceded by a consonant but not followed by another.

The preceding observations show that the finger beat occurs before the vowel. But where does it come in respect to a consonant which precedes the vowel?

Among the three consonants *m*, *p* and *h* which formed the objects of our experiments in combination with the vowel *a*, the last two (*p* and *h*) could be found in the records. The curve for the consonant in our records did not consist of vibrations like those of the vowel, but of a smooth deviation from the record line due to the air pressure. The lengths of the consonants could thus be measured. The results of the measurements are given in Tables XXXII and XXXIII. The unit of the measurement is $\sigma = 0.001^s$.

A summary is given in Table XXXIV.

Table XXXII.

Finger beat with 'ha.'

Subject.	Average length of *h*.	Immediate probable error.	Average time of beat before *h*.	Number of measurements.	Number of +.	Number of —.	Immediate probable error.
E	43	5.1	+ 67	10	10	0	7.5
	34	9.0	+ 53	10	10	0	12.0
	39	6.1	+ 34	10	10	0	9.4
K	109	13.1	— 67	10	0	10	27.2
	124	15.4	— 30	10	1	8	18.9
	148	7.4	— 88	10	0	10	33.3
	99	21.0	— 64	10	0	10	27.0
	106	21.4	— 62	10	0	10	32.4

Unit of measurement, $\sigma = 0.001^s$.

Table XXXIII.

Finger beat with 'pa.'

Subject.	Average length of *p*.	Immediate probable error.	Average time of beat before *p*.	Number of measurements.	Number of +.	Number of —.	Immediate probable error.
E	54	7.6	+ 91	10	10	0	17.6
	40	3.4	+ 49	10	10	0	10.9
	47	4.7	+ 87	10	10	0	18.6
	47	5.8	+ 61	10	10	0	14.5
	53	9.3	+ 56	10	10	0	15.0
K	56	6.2	+ 16	10	7	2	15.2
	60	8.1	+ 5	10	7	3	18.9
	57	26.0	+ 29	10	9	1	19.3
	73	33.3	[illegible] 5	10	5	5	18.2
	51	7.7	+ 11	10	6	4	7.0
	55	10.6	+ 25	10	8	2	19.5

Unit of measurement, $\sigma = 0.001^s$.

Table XXXIV.

Finger beat with rhythm of speech.

Subjects.	Average length of *p*.	Average time of beat before *p*.	Average length of *h*.	Average time of beat before *h*.
E	49	+ 69	39	+ 51
K	5[illegible]	+ 15	117	— 61

Unit of measurement, $\sigma = 0.001^s$.

The following points may be observed in the tables:

1. For the syllable *pa* two subjects agree in beating time before the beginning of the consonants.

2. For the syllable *ha*, with the subject E the beats come constantly before the beginning of the consonant, but with K they come in most

cases after the beginning of the consonant, about midway between the consonant and the vowel which follows it.

From the observations reported in this section, the final conclusion can be drawn that the beat of the finger in connection with the rhythm of speech comes before the vowel and before or in the course of the consonant which precedes the vowel.

The preceding observations would not be complete unless a few words are added about the point of emphasis in rhythmic articulation. We would naturally raise a question as to the relation of the beat of the finger to the point of greatest emphasis.

Our experience seems to show that when we recite a verse while we beat time with our hand, the point of the highest emphasis in the rhythm comes at the same moment with the beat.

Although it is not certain whether the innervations of the movements of hand and vocal organ proceed from their nervous centers at exactly the same moment, still we may suppose that the two movements are so closely associated that the innervations of them take place almost simultaneously. But when we attempt to determine the position of the point of emphasis from the beat of the finger, we find that it cannot be easily done. It does not follow that the movements themselves are executed at the same time from the mere supposition that innervations of the movements of hand and vocal organs take place simultaneously.

MEYER[1] supposed that the movements of hand and vocal organ would take place at the same moment, provided the nerve fibers which transmit the impulses are equal in length. He calculated from the rate of nervous transmission that the impluse reaches hand 1.47 hundredths of a second later than vocal organ. Adding the latent time of apparatus to this lost time of nerve transmission he arrived at the final conclusion that the point of emphasis lies in the course of a voiced consonant or shortly before an explosive.

The difference in the length of the nerve fibers is not the only factor which disturbs the simultaneity of the two movements. KÜLPE's experiments[2] showed that we have difficulty in moving our hands at the same time to react to a single stimulus. If even the two hands—alike in construction and symmetrically arranged—are not moved simultaneously, it must be still more difficult to execute the movements of two disparate organs like hand and vocal organs at the same moment.

Besides these differences there may be several other factors which cause the deviation of the two movements. The difference of the complexity

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[1] MEYER, *Beiträge zur deutschen Metrik*, Neuere Sprachen, 1898 VI 121.

[2] KÜLPE, *Ueber die Gleichzeitigkeit von Bewegungen*, Philos. Stud., 1891 VI 514.

of the constructions of the two organs, might be one of such factors. The condition of attention during the movements might be another.

Therefore, until all the conditions on which the simultaneity of the two movements depends are known, nothing definite can be said about the relation of point of emphasis to the finger beat.

If we assume, however, that the movements of the hand and vocal organs are executed simultaneously, we can conclude from the foregoing experiments that the point of emphasis in the rhythmic speech comes before the vowel and before or in the course of the consonant which precedes the vowel. In other words, the point of emphasis in rhythmic articulation lies at the *beginning* of the movement of the vocal organs for the production of the sound.

RESEARCHES IN EXPERIMENTAL PHONETICS

(Second Series)

BY

E. W. Scripture.

These researches are a continuation of the first series, published in these Studies.[1]

I. Apparatus for studying speech records.

The apparatus for transcribing gramophone records[2] has been so developed that the curves are much larger; those shown in Plates I to XI are reproduced directly by photography without any magnification.

The tracing apparatus in the form used for transcribing the records reproduced in these plates is partly shown in a top view in Fig. 1 and in a side view in Fig. 2. The gramophone plate *E* (Fig. 1) is placed on a metal disc (Fig. 2) which is rotated about once in five hours by miter gears connected to the screw barrel in the tube *C* (Fig. 1). This tube is turned by a spur gear *Y* which is moved by a speed reducing mechanism from an electric motor. As *C* revolves, it turns the screw barrel and the gramophone plate; at the same time the screw barrel moves longitudinally through a nut and pushes the plate to the left. The iron plate *D* forms the base of the apparatus. A steel point near *F* in the lever *J*, held by the adjustable support *H* on the base *I*, runs in the speech groove on the gramophone plate. The lever *J* thus repeats the horizontal vibrations in the speech groove. The movements are transferred to the second lever *Q*, working on a fulcrum *O* supported by *P*, by means of the link and gimbal joints *L*, *N*. The movements of *Q* are registered by a point *R* on a band of smoked paper *S* stretched between two drums, of which one is shown at *T*. The drum *T* is moved by a belt from the pulley *X*. The speed of the gramophone plate and that of the drum are thus always in a constant ratio.

The magnification of the vibrations in the speech groove can be of any degree, provided the mechanical working is sufficiently accurate. The following technical points were learned from long and costly experience.

[1] Scripture, *Researches in experimental phonetics (first series)*, 1899 VII 1.

[2] Scripture, as before, 10.

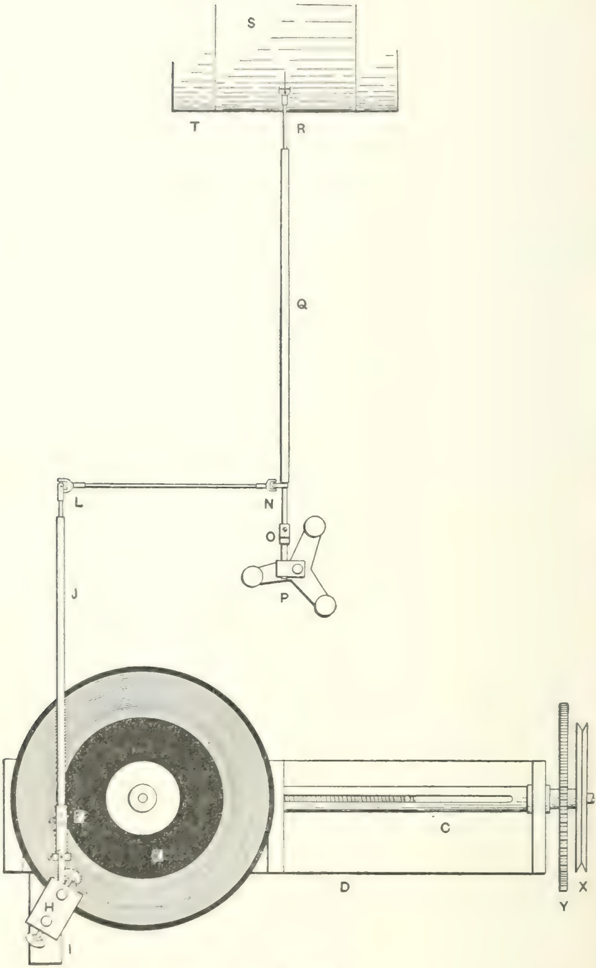

Fig. 1.

The bearings for the levers *J* and *Q* must be perfectly tight and yet perfectly loose; the slightest bind or play destroys the records. To attain this condition all the pivot bearings are made of steel highly polished under a magnifying glass. All parts are made with the utmost lightness and rigidity. The arms *J* and *Q* are of a reed specially imported from Japan; I have been unable to find any other substance that will give equal rigidity with so little weight. The link *LN* is a small reed from Germany used for marking instruments; it has great longitudinal strength, that being all that is required in the application.

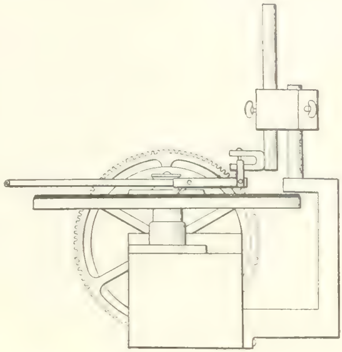

FIG. 2.

The recording point *R* was sometimes of steel in an aluminum holder (as shown in the figure) or was what is known as the BAYLIS recording point. This latter tracing point is worthy of a description. I made my specimens at a suggestion from a medical man; as I have never seen the original account, I cannot tell how widely they may differ. The construction of the point is shown in Fig. 3. A piece of thin card is cut into two portions, and the two are united by the thinnest obtainable rubber membrane (I use some obtained from KŒNIG, of Paris, for manometric flame capsules);

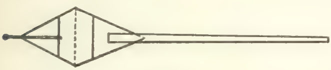

FIG. 3.

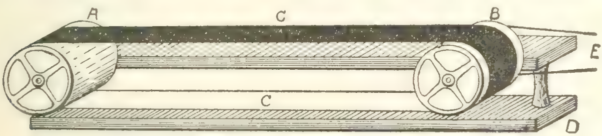

FIG. 4.

this forms an exceedingly delicate hinge. A fine glass thread is made; a piece is broken off; one end is melted to a little ball; and the piece is cemented to the free piece of cardboard. The other piece is attached to

the recording arm. The weight of the free piece keeps the point on the paper; the hinge allows the necessary play.

In order to use long bands of paper the recording drums must be conveniently arranged. One arrangement is shown in Fig. 4. Two plates *DE* are held together by crossrods. At any points on the edges of these plates metal shafts may be clamped, and two drums *A B*, with hollow axles, placed on them. A band of paper *CC* is fastened evenly around the drums and tightened after the paste is dry by adjusting one of

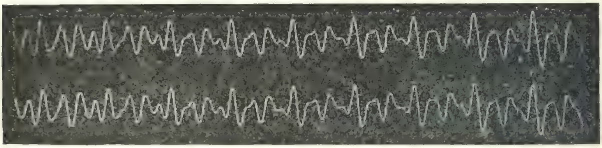

Fig. 5.

the shafts; it is then smoked as usual. To rotate the drums a loose pulley may be placed on one of the shafts before or after the drum is on the support; this pulley has a pin that catches one of the spokes of the drum.

The accuracy with which the machine reproduces the vibrations in the groove on the gramophone plate may be shown by a comparison of repeated tracings of the same curve; the pieces in Fig. 5 were cut from different tracings and were reproduced directly by photography. The tracing is thus done with an accuracy indicated by the likeness of the two records. These differences are so small as to escape anything but microscopic measurement. The fine vibrations in the consonants and some of the vowels, which are lost in the tracing, are smaller than these differences.

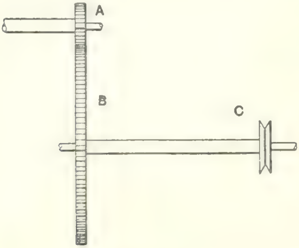

Fig. 6.

As this machine can be run continuously day and night with no supervision except for changing the paper, great quantities of tracings can be accumulated in spite of the low speed.

The problem of reducing the speed of an electric motor to any degree I have solved in the following general way:

An adjustable countershaft fastened to the base of the motor allows the speed to be reduced in transmission. For very high speeds the belt from a pulley on the drum runs directly to a small pulley on the motor axle. For more moderate speeds the countershaft is used with a spur gear A on the motor axle and another B on the countershaft (Fig. 6); the pulley C on the countershaft runs at a lower speed on account of the reduction AB. For very low speeds a worm W is placed on the motor axle and a worm gear V on a spur gear on the countershaft (Fig. 7). When the drum is used with its axis horizontal, a spur gear S (Fig. 8) may, if preferred, be placed on its axle and made to connect with a spur gear T of any desired size on the countershaft, which is run by a worm W on the motor axle. For very low speeds the spur gear S (Fig. 9) is run by a worm X on the countershaft, which is turned by the worm gear V in connection with the worm W on the motor axle. A collection of various sizes of pulleys and gears makes it possible to get almost any speed desired; the finer gradations are accomplished by resistances and by slightly pressing or loosening the motor brushes against the commutator.

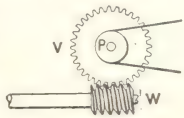

FIG. 7.

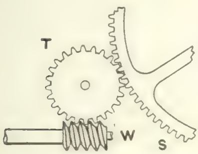

FIG. 8.

For reproducing the tracings the following procedure has been found successful: A narrow strip containing the curve is cut from the smoked paper. This strip is then placed so that it reads from right to left. A piece the length of the desired plate is cut off and pasted on pasteboard. Below this another length is pasted; and so forth until the plate is of the desired height. The edges and open spaces are then blackened. The engraver uses this as copy for making the block, but *omits the process of stripping* (*or reversing*). The print from the block thus made will read from left to right. The omission of the stripping avoids the errors due to stretching of the gelatine.

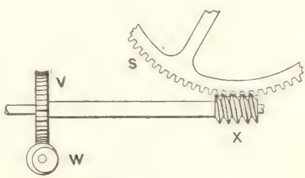

FIG. 9.

II. Interpretation of speech curves.

A curve of speech is at first sight no more intelligible than a line of Chinese ideograms. The knowledge of the speech sounds to which a

certain portion of a curve belongs gives the general meaning of the curve but affords little information concerning its details. A careful study of the sound by the ear reveals some of the grosser characters of the sound, but cannot indicate any of the finer details that lie before the eye in the complexities of the curve. The meaning of these details—the very essentials of the speech sounds—is not apparent at first observation; only by patient and persistent unraveling of the tangled curve is an inkling of it obtained.

The experience of several years has developed a method of studying the tracings from the gramophone (or zonophone) discs that aims to save some of the great amount of time involved.

The words spoken by the gramophone plate are noted on paper with an indication of the relative lengths of the pauses. The pauses are classed as short, medium and long.

The first vibrations on the record are taken as representing the first word on the plate. The first long straight line on the record is taken as the first pause. Then the successive sounds between the beginning and the first pause are assigned to the successive groups of vibrations. This method is followed for succeeding groups of sounds between pauses. Considerable help is obtained by a familiarity with the peculiarities of speech curves.

A set of speech curves (Plate I) from the *Cock Robin* record will be used to illustrate the first steps taken in analysis. The curve reads from left to right; the italicized letters indicate the sounds recorded.[1] The speech curves in the figure would naturally run along horizontal lines. The slow fluctuations seen in the records are due to irregularities in feeding the gramophone plate sidewise. They in no way affect the accuracy of the records. In making measurements of duration, however, the ruler should always be horizontal.

To interpret the details of a sound the grouping of the vibrations is first noticed. In a series of groups of the same general form each group may usually be considered as arising from one puff of the vocal cords. The minor vibrations arise from the vibrations of the resonating cavities and from the overtones of the cords.

Many of the main features of the speech curves can be obtained by inspection without measurement; very much more can be obtained by simple measurements. Long distances may be measured by millimeter scales; the tenths of a millimeter may be estimated by the eye. Finer measurements may be made with a scale graduated in tenths of a milli-

[1] This account is from SCRIPTURE, *Speech curves, I*, Mod. Lang. Notes, 1901 XVI 71.

meter;[1] the work is done with a watchmaker's eyeglass, or under a magnifying glass. When the curves are very small, the measuring may be done by a microscope with a micrometer object-table or a micrometer eye-piece.[2]

The calculations are all done by books of tables[3] or with a slide rule.[4] The investigator should become familiar with various books containing extensive multiplication tables, tables of reciprocals, etc. A Chinese abacus is also very convenient in adding.

The speech curves are frequently of such a nature that the period of the cord tone may be found by measuring the distance between two like points in two successive groups of vibrations.

The distance in millimeters is translated into time according to the equation valid for the tracing. For all the curves in Plate I except that of "draw your" the relation is $1^{mm} = 0.0016^{s}$; for this curve it is $1^{mm} = 0.0007^{s}$. Thus, the distance between the two high points in the last vibration in the fourth line is 3.2^{mm}; at 1^{mm} for 0.0016^{s} (use ZIMMERMANN's table for 16) this gives a period of 0.01536^{s} for the cord vibrations at that instant. A period of 0.01536^{s} is the same as a frequency of $1 \div 0.01536$ (use BARLOW for reciprocals) or 65.1.

To illustrate the method a detailed analysis of the words "saw him" will be given in the next section.

III. FURTHER STUDIES OF *Cock Robin*.

The *Cock Robin* record previously described[5] was traced off again with the apparatus shown in Fig. 1. The curves were much larger than the previous ones. Those in Plates I and II are reproduced directly by zinc etching (p. 53) with no enlargement.

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[1] For measuring rules: SOCIÉTÉ GENEVOISE, Genève (especially adapted is a 'petite échelle en argentan divisée d'un côté en dixièmes de millimètres' for 20 francs).

[2] For microscopes with micrometer eye-pieces: ZEISS, Jena; BAUSCH & LOMB, Rochester, N. Y. For micrometer object tables: ZIMMERMANN, Leipzig.

[3] For mathematical tables: CRELLE, Rechentafeln, Berlin, 1857 (first English edition, New York, 1888); ZIMMERMANN, Rechentafeln, Berlin, 1891; BARLOW, Tables of Squares, Cubes, Square Roots, Cube Roots, Reciprocals of all Integer Numbers up to 10000, reprint edition, London, 1897.

[4] For slide rules and similar calculating instruments: DENNERT & PAPE, Altona; W. F. STANLEY, London; BEYERLEN & CO., Stuttgart; TAVERNIER-GRAVET, Paris; KEUFFEL & ESSER, New York. For adding machines: FELT & TARRANT, New York City. For calculating machines (most advantageous for multiplication and division): BURKHARDT, Glashütte i/S.; BRÜCKNER, Dresden; GRIMME, NATALIS & CIE., Braunschweig. For the curve-adder: CORADI, Zürich.

[5] SCRIPTURE, *Researches in experimental phonetics* (*first series*), Stud. Yale Psych. Lab., 1899 VII 14.

The first line on the Plate[1] contains the record of *ɔhim* in *sɔhim* which occurs in the phrase "Who saw him die?" The words are run together in speech on the gramophone, so that there is no pause between *ɔ* and *h*.[2]

The record shows no trace of the *s*. The first vibrations of the curve differ from the rest, and show changing relations between the resonance (or mouth) tone and the cord tone; they indicate that the cords have begun to vibrate while the mouth is still changing from the *s* position to the *ɔ* position. After this the grouping of the vibrations in threes indicates a cord tone with a cavity tone a duodecime higher; this general relation is maintained throughout the vowel. That still other cavity tones are present is indicated by the subordinate modifications of the small vibrations. The sound *ɔ* increases slowly in intensity, but diminishes again as it changes into the following sound. The *i* is quite strong but falls quickly as the sound changes to *m*. The *m* vibrations slowly fade away.

The accompanying table shows the way in which the course of the cord tone in reference to pitch is calculated. It illustrates several important principles used in computing and interpreting results.

The figures in column *A* give the distances in millimeters from apex to apex of the strongest vibrations in the successive groups. The measurements were made by an assistant who did not know the nature of the problem investigated. It is very important to note the following:

1. The determination of the exact point to be called the apex may be indefinite to the extent of one or two tenths of a millimeter, owing (*a*) to the roundness of the apex, (*b*) to the fact that the apex is sometimes slightly displaced by interfering cavity tones.

2. The general character of muscular action forces us to assume that the changes in the voice proceed with some regularity; this would indicate that the unusual figure 2.6 for the sixth period does not give the proper period at that point but shows something else.

Using ZIMMERMANN's table for 16, the figures in column *A* are turned into time by the equation $1^{mm} = 0.0016^{s}$, with the results given in column

[1] This Plate and much of the analysis were first published in SCRIPTURE, *Speech curves, I*, Mod. Lang. Notes, 1901 XVI 72.

[2] The system of phonetic notation is that used in SCRIPTURE, Elements of Experimental Phonetics, New York 1902.

A Period in millimeters.	*B* Period in seconds.	*C* Frequency.	*A* Period in millimeters.	*B* Period in seconds.	*C* Frequency.
3.8	0.0061	167	4.8	0.0077	130
3.8	0.0061	167	5.0	0.0080	125
3.9	0.0062	161	5.1	0.0082	122
4.0	0.0064	156	5.0	0.0080	125
4.0	0.0064	156	5.1	0.0082	122
2.6			5.2	0.0083	120
4.2	0.0067	149	5.1	0.0082	122
4.2	0.0067	149	4.7	0.0075	133
4.1	0.0066	152	4.6	0.0074	135
4.0	0.0064	156	4.7	0.0075	133
4.2	0.0067	149	4.8	0.0077	130
4.3	0.0069	145	4.7	0.0075	133
4.3	0.0069	145	4.4	0.0070	143
4.2	0.0067	149	4.5	0.0072	139
4.3	0.0069	145	4.5	0.0072	139
4.3	0.0069	145	4.5	0.0072	139
4.3	0.0069	145	4.7	0.0075	133
4.1	0.0066	152	4.5	0.0072	139
4.2	0.0067	149	4.7	0.0075	133
4.3	0.0069	145	4.5	0.0072	139
4.5	0.0072	139	4.6	0.0074	135
4.5	0.0072	139	4.4	0.0070	143
4.5	0.0072	139	4.6	0.0074	135

B. These are the lengths of successive periods in the cord tone. Using a table of reciprocals (Barlow or Zimmermann) these are turned into frequencies by the equation $C = 1/B$, with the results given in column *C*.

The curve of frequency is now to be plotted. This is best done by supposing the speech curve to be laid off along the horizontal or X axis, so that the first vibration is at zero. Above zero the proper number of millimeters is counted upward to indicate the frequency of the cord tone at the start. Thus, if the period of the first group is 0.12^s, the frequency will be 83; if 100^{mm} have been assigned to each 100 of frequency, the dot will be placed at 83^{mm} above the X axis. Above the point on the X axis at which the second group of vibrations would begin if the curve were laid upon it, the frequency of the cord tone at this moment is indicated by a dot at the proper height. In this manner a series of dots is obtained, indicating the frequency of the cord tone at a succession of moments. (Plate XIV, Fig. 1.)

In the diagram of frequency the successive dots might be connected by straight lines. We probably come nearer to the true curve of frequency by drawing a smooth curve that evenly distributes the dots on either side. This may be done with the free hand, by means of draughtsman's curves, or by a flexible rubber ruler ; the more general reasons for this procedure may be found in works on the methods of science.[1] The curve of frequency of *ɔhi*, plotted from the table on p. 57, is shown in Plate XIV, Fig. 1.

The curious interruption of the regular course of figures in the table by 2.6 arises from the fact that the series of the strongest vibrations used to mark off the groups is replaced at this point by a series arising from one of the weaker vibrations. In the first part of the curve there is some vibration of a changing character that causes a change in the moment of strongest vibration. The unusual figure indicates this latter fact and not any sudden break in the cord tone. A similar occurrence may be seen in *o* of "bow" at the middle of line 2 (Plate I) and in *ɔ* of "draw" as indicated below.

The periods of the smaller, or cavity, vibrations can frequently be obtained by direct measurement. This occurs most readily when these vibrations are of a simple form or of a pitch much higher than the cord tone. The result becomes more accurate when several successive cavity vibrations can be measured together. When the cavity vibrations are simple in form and a place in the curve can be found where a number of them exactly fill out a group period, the length of the group period divided by the number of vibrations will give the length of the cavity period.

No detailed study of the specific sounds will be undertaken on the present occasion ; this will be done in the near future, as soon as the enormous labor of analyzing the similar sounds of several speakers has been completed. One sound, however, calls for special attention, namely, the sonant *h*.

A faint *h* is distinctly heard between *ɔ* and *i* in "saw him" ; the observation has been verified by several listeners. There is no interruption of the vibrations between the two vowels, but a slight weakening occurs near the middle of the record. The *h* is thus a sonant one. Other cases are to be found in "saw him" of Plate II and "had" of Plate VII. PIPPING[1] records a similar case in a record of Finnish "keihäitä."

[1] JEVONS, Principles of Science, Chap. XXII.

[2] PIPPING, *Zur Phonetik d. finn. Sprache, Unters. mit Hensen's Sprachzeichner*, Mém. de la Soc. finno-ougrienne, XIV, Helsingfors 1899.

Sonant *h* was regularly prescribed by the Sanskrit grammarians.[1] It is used in some modern languages.[2]

The curves of the vowel sounds of the word "bow," of the phrase "with my bow and arrow," are shown in the second and third lines of the Plate. To the ear the word appears melodious and prolonged; it might even be called mellifluous.

The tracing begins with three faint vibrations that presumably occur as the mouth begins to open after the occlusion for *b*. Thereafter the vibrations follow in groups of four, beginning with a length of 5.5^{mm} and decreasing slowly to 4.8^{mm} in the middle of the line; this indicates a cord tone of rising pitch. The cavity tone remains practically constant at 1.5^{mm} per vibration, or a period of 0.0024^{s}, or a frequency of 417.

The amplitude rises steadily to a degree that indicates considerable loudness; it then falls rather suddenly (middle of second line in Plate I). The vibrations beyond this point show so many peculiarities that their difficulties can best be attacked by working backward from a later point where the grouping is more regular. Somewhat beyond the middle of the second line in Plate I the vibrations fall into groups having two main crests with two subordinate ones. This entire group arises presumably from one cord vibration. This conclusion is drawn because further on to its right the group gradually changes to two main crests only, a typical form for a cord tone accompanied by a cavity tone nearly an octave higher. This condition of a cord tone with an octave cavity tone is modified in the first part by higher tones that do not form an exact harmonic interval with either of the other tones; these give rise to the minor fluctuations in the middle of the line. These higher tones are of changing pitch, as can be seen by the steadily changing form.

The puffs of air from the cords are not generally of the even nature found in sinusoid vibrations;[3] they rather resemble more or less sharp explosions.[4] In this sound they are not so sharply explosive as in *au* of "shroud" or *æ* of "sparrow," yet the puff has its greatest intensity in the first part of the interval of time it occupies.

[1] For examples see Tâittirîya Prâtiçâkhya, II. 47, ed. by WHITNEY, Journ. Amer. Oriental Soc., 1871 IX 77.

MICHAELIS, *Über das H und die verwandten Laute*, Arch. f. d. Studium d. neueren Sprachen (Herrig), 1887 LXXIX 49, 283.

[2] MEYER, *Stimmhaftes H*, Neuere Sprachen, 1900 VIII 261; *tsum ſtimhaftn ha*, Maître phonétique, 1901 XVI 87.

KLINGHARDT, *Stimmhaftes H*, Neuere Sprachen, 1901 IX 85; PASSY, *H vocalique*, Neuere Sprachen, 1901 IX 245.

[3] SCRIPTURE, Elements of Experimental Phonetics, 2, New York 1902.

[4] SCRIPTURE, as before, 260.

Starting from the strong vibrations (third quarter of line 2), we mark off backward the alternate higher vibrations as the points of maximum for each cord puff. We thus have the vibrations in pairs; the period of the cord tone at any moment will be given by the distance between two such marked vibrations.

As we go towards the left, we see that each of the vibrations of a pair shows a tendency to split up into two minor vibrations; this indicates the presence of higher cavity tones. Measurements of the periods of the cord tone show that it steadily rises in pitch (Plate XIV, Fig. 2).

The alternate (or cavity) vibration keeps very closely at the middle of the cord period; though in the first portion it is generally a little behind the middle point. This indicates a cavity tone in general an octave higher than the cord tone, but a little lower in the first portion. The details can be brought out by measurements.

In addition to the two maxima of amplitude in line 2 there is a third maximum in line 3. It may be suggested that perhaps this vowel sound is to be considered as a triphthong. Careful listening to the gramophone plate enables the ear to hear two maxima clearly and the third faintly. The maxima are due, not to any breath emphasis, but to coincidence of the cavity period with a submultiple of the cord period.[1]

The word "shroud" occurs in "Who'll make his shroud?"

One pseudobeat for the *r* occurs at the flat place in line 4. The vibrations in line 3 and at the beginning of line 4 belong to the vowel-like sound in connection with which the flaps of the *r* occur. After the occlusion of the pseudobeat the tongue again allows the cord- and cavity-vibrations to appear. The form of the vibration is different, indicating a changing adjustment of the mouth from the *r* position to the *a* position; this position is to be considered as the *r-a* glide. There is no possibility of limiting the *r* from the *a*, or of marking off a distinct *r-a* glide; the change is gradual throughout.

The *r-a* glide after the flap is followed by the long record for *au* reaching to the middle of line 5. The latter part of line 5 contains the faint vibrations of the *u-d* glide, the still fainter ones of the *d*-occlusion, and the strong ones of the *d*-explosion.

The curve of frequency is shown in Plate XIV, Fig. 3. During *au* the cord tone rises from 120 in frequency to 111 and then falls steadily to 92. The diphthongized vowel *au* is thus of circumflex pitch. In *d* the cord tone rises to 109.

The *au* is of crescendo-diminuendo intensity, the crescendo being gradual and the diminuendo rather sudden.

[1] SCRIPTURE, as before, 13.

The word "sparrow" occurs in "I, said the sparrow." The *æ* begins at the first quarter of line 6. The first few cavity vibrations show a changing form as the *p* glides into the *æ*. The *æ* ends at the last quarter of line 6; here the cavity vibrations again change their forms as the *æ* becomes *r*. The *r* has one flap. This does not produce absolute silence, as some vibrations can still be detected in the tracing. The very long *o* extends over the last fifth of line 6 and nearly all of line 7. In general the curve of this *o* differs considerably in the details of the cavity vibrations from that of *o* in "bow" (above); it has none of the large and sudden changes in amplitude.

The curve of frequency of the cord tone is shown in Plate XIV, Fig. 4. The cord tone starts at *æ* with 125 in frequency, rises to 202, and then falls slowly to 136 at the end of *o*. The amplitude of *æ* increases slowly, then falls suddenly, and becomes almost zero at the end of the *æ-r* glide. The amplitude of *o* increases quite rapidly and continuously during the *o* to a maximum beyond which it gradually decreases as the vowel fades away in its gradual exit.

The words "draw your" occur in the introduction "Now, children, draw your little chairs nearer." The last five lines give the curve for nearly all of *ɔju*, omitting a piece at the end. The recording surface was run at a greater speed than for the previous curves; the space-time equation is $1^{mm} = 0.0007^{s}$. This speed is more favorable for the details of vibrations of greater amplitude but less favorable for those of smaller amplitude.

The analysis of the curve may be approached in the following way. The vibrations in the latter portion of the eighth line are evidently to be grouped in threes. There is present here a cord tone with a cavity tone a duodecime above it. The last group on this line has a length of 10.2^{mm}, indicating a cord period of 0.0071^{s}, or a frequency of 143. Measuring backward we find that the preceding group is a little longer than this one; in fact each group is found to be a little longer than the following one. The cord tone is thus shown to be rising in pitch.

The three small vibrations that make up the last group on line 8 are nearly equal in length although the last one appears to be cut off somewhat by the following stronger vibration of the next group. The preceding group shows nothing of the cutting off. The next preceding group shows that the three small vibrations do not quite fill out the interval between the apexes of two strong vibrations selected to mark off the groups. This becomes still more evident in the further preceding groups. This condition seems to indicate that the small vibrations composing a group retain a constant period while the length of the

group is changing. In confirmation of this we finally find four small vibrations instead of those in the early part of the vowel. The period of the small vibrations is approximately 0.0028^s, giving a frequency of 357. This is a very clear illustration of the fact that the cavity tones of vowels are independent of the cord tone in regard to pitch, and are not overtones of it as commonly supposed.

That there are still other resonance tones is indicated by minor deformations of the curve, but further information concerning them is not obtainable at present.

Proceeding onward, we find that the cord tone continues to rise. At the first quarter of line 9, the length of a group is 9.0^{mm}, giving a period of 0.0063^s, or a frequency of 159; at the third quarter the length is 8.0^{mm}, the period 0.0055^s and the frequency 179. The tone now rises more slowly. At the first quarter of line 10 the length is 7.5^{mm}, the period 0.0053^s, and the frequency 189. Beyond this point the tone remains nearly constant.

In the meantime the cavity vibrations have been undergoing a change. Instead of one cavity tone, two begin to show themselves distinctly. The most powerful one appears as a fairly strong vibration at 50^{mm} (0.0035^s) after each strongest vibration in the group. Although the group shortens, this vibration remains at a nearly constant distance from the beginning, necessarily, however, approaching closer and closer to the end of the group. The strong secondary vibration has been observed[1] in many cases of *a* in *ai*. In those cases it remained at a constant distance from the beginning of the group till the group became so short that it coalesced with the strongest vibration of the following group. Here the result is different. Instead of remaining at an absolutely constant distance behind the preceding strongest vibration of the group, it gradually, but not greatly, lessens the distance till, as the cord tone becomes stationary in pitch, it ultimately occupies the middle of the group as the octave of the cord tone. But another change has taken place that is of a puzzling nature; this strong secondary gradually becomes stronger than the other vibrations in the group. This can be readily seen by checking off the strongest vibrations in line 9 as boundaries of groups beginning at the left; in the middle it will be found that one vibration has become stronger than the ones that must be checked off as boundaries of groups. An explanation of this phenomenon is lacking.

The cord tone remains constant with a period of about 0.0053^s throughout line 10. The cavity tone at an octave above also remains

[1] SCRIPTURE, *Researches in experimental phonetics* (*first series*), Stud. Yale Psych. Lab., 1899 VII 26.

unchanged. The other cavity tones that produce the small marked inflections in line 9 and line 10 gradually die away, leaving the vibrations grouped in pairs at the end of line 10.

In line 11 the vowel somewhat suddenly decreases in amplitude. It is followed by the small vibrations of the weak (but not very short) *j* that precedes *u* in "your."

Line 12 shows the vowel *u* of "your";the curve is not completed.

The curve of frequency is given in Plate XIV, Fig. 5. The cord tone rises from about 75 in frequency at the beginning of *ɔ* (line 8) to about 189 (line 10), after which it remains practically constant until it begins to fall in the *ɔ-j* glide (last part of line 10). During *j* and *u* the tone falls steadily.

The curves in Plate II are from the words: "I" in "I, said the beetle," "my" in "with my bow and arrow," "parson" in "I'll be the parson," "saw him" in "I saw him die," "caught" in "Who caught his blood?" and "said" in "I, said the rook."

The curve for "I" shows a series of vibrations in which each group resembles the neighboring one, while there is a gradual change in character from a typical form for the *a* in the first part to a typical form for the *i* in the second part of the diphthong *ai* of which the pronoun "I" is composed. In the first portion there appears a succession of strong vibrations, each followed by a series of weaker ones. These strong vibrations recur at periods of steadily decreasing length.

If we consider separately each group of vibrations beginning with a strong one, we find that it is, aside from minor details, the typical curve[1] of a vibration initiated by a blow and dying away by friction, for which the equation is

$$y = a \cdot e^{-kt} \cdot \sin 2\pi \frac{t}{T},$$

where y is the elongation at the moment t, a the amplitude, e the basis of the natural series of logarithms, k a factor representing friction and T the periodic time.

The succeeding groups of vibrations following the first group are of the same form but of steadily increasing amplitude. They recur at steadily decreasing intervals. The formula for each group is approximately the same except for the difference in amplitude. The vibrations are evidently aroused by a series of blows of steadily increasing strength at steadily decreasing intervals.

It seems clear that these vibrations represent the free vibrations of the

[1] SCRIPTURE, Elements of Experimental Phonetics, 6, New York 1902.

air in the mouth cavity aroused by a series of sudden blows and that these sudden blows are due to explosive openings of the vocal cords.[1]

The tone from the cords results from the succession of groups of vibrations ; it is a tone of intermittence. The period of the tone from the cords is represented by the distance from the strong vibration at the beginning of each group to the strong one at the beginning of the following group.

The complexities of the small vibrations indicate the presence of several partial tones. These complexities change steadily from the beginning of the vowel onward as the pitch rises, in a way to indicate the presence of at least the following partials : 1. the fundamental cord tone consisting of a series of explosions rising from a period of 0.0170^s (frequency, 59) to one of 0.0052^s (frequency, 192); 2. a constant cavity tone of 0.0034^s period (frequency, 294) shown by the large secondary ; 3. a constant cavity tone of 0.0013^s period (frequency, 769) shown by the smaller vibrations, and 4. higher cavity tones undergoing change.[2]

The minor complexities in the vibrations disappear at about one-quarter of the distance from the left on the second line in the figure. At the same time the amplitude is strongly increased. Shortly afterward the amplitude decreases and finally reaches zero. Throughout the whole latter portion the curve has an entirely different character from that of the first half ; we are probably quite safe in considering it the curve of *i* in the diphthong *ai*. Throughout the *i* the groups consist of two vibrations, one slightly stronger than the other. The period for the group 0.0052^s (frequency, 192) remains constant till near the end, where it lengthens to about 0.0122^s (frequency, 82). The cavity vibration forming half of each group remains constant at 0.0026^s (frequency, 384) through nearly all of the *i*. Toward the close it apparently still remains at the same period, producing phenomena of interference as the group period is lengthened.

From the curve for *i* it seems justifiable to conclude that the vocal cords emit explosions instead of sinusoid puffs of air here as well as in the *a*. The explosion produces a strong free vibration in the mouth cavity which is followed by another of diminished amplitude. This would be followed by a third of still less amplitude, just as in *a*, but a new explosion from the cords occurs at just that moment. The coincidence of double the period of the cavity tone with the period of the cord explosions explains the rapid gain in amplitude when the cord tone rises sufficiently to produce the coincidence (p. 62). The maximum is followed by a relaxa-

[1] SCRIPTURE, as before, 260.

[2] SCRIPTURE, as before, 91.

tion in the force of breath, but the two tones maintain the same relation for a considerable time. As the sound finally dies away, the cords also relax, both breath and pitch falling together. The explosions from the cords seem much less sharp in *i* than in *a*.

In "my" the *m* vibrations are too faint for accurate measurement. The *a* resembles somewhat, but not closely, the *a* of "I." The period of the cord explosions remains constant at 0.0074^s (frequency, 135) instead of decreasing. The lower cavity tone has a period in the neighborhood of 0.0022^s (frequency, 455); it apparently undergoes a slow change from the beginning of the *a* to the *i*.

The last third of the curve somewhat resembles the *i* portion of "I." There is, however, only a faint rise in amplitude, and the *i* portion is very brief. The vibrations in this portion are in groups of three; the groups have a period of 0.0074^s (frequency, 135) constant to the end. The vibrations within the group have a period one-third that of the group itself, indicating a constant cavity tone of 0.0025^s (frequency, 400).

In the *a* of "parson" the cord tone rises from a period of 0.0090^s (frequency, 111) to one of 0.0072^s (frequency, 139) and falls again to the pitch from which it started. There are indications of a constant cavity tone of 0.0022^s (frequency, 455) and of higher tones with changing periods. In respect to the pitch of the lowest cavity tone there is close agreement of this *a* with that of "my," yet the form of the curve resembles that of *a* in "I" more closely than that in "my." The peculiarity of "my" seems to lie chiefly in the suddenness with which the vibrations within a group fall in amplitude after the initial strong vibration. In both "parson" and "I" the cavity vibrations within each group during *a* die away less quickly. Such differences may perhaps find their explanation either in the greater friction on the free vibratory movement in the mouth (less rigidity of the walls?) or in the sharper character of the cord explosions in the case of "my."

The curve for *ɔ* in "saw him" indicates a quite different vocal action from that present in *a*. Instead of a strong initial vibration followed by decreasing ones the earlier portion of the vowel shows groups that contain at least two strong vibrations. It is presumably the case that the cord explosions are of a more gradual character or else that the action of friction is much less. Even later in the vowel where there is apparently only one very strong vibration in a group, this probably occurs because the lower portion of the second one is cut off by interference with another partial tone.

The cord tone, starting with a period of 0.0072^s (frequency, 179), remains at this pitch for a time and then falls to 0.0080^s in period

(frequency, 125). The lower cavity tone with a period of 0.0026^s (frequency, 385) is apparently present.

The last part of the line shows the vibrations for *i*, resembling those for *i* in *ai* of "I" and "my." The middle portion, where there is a weakening in amplitude, belongs to the sonant *h* (p. 58). The *m* is just begun where the record is cut off. The grouping in the *i* is by threes. The cord tone of *i* starts with a period of 0.0083^s (frequency, 121) and steadily rises to one of 0.0072^s (frequency, 139) in the *m*. The lower cavity tone has a period of about 0.0025^s (frequency, 400).

The curve for the *ɔ* of "caught" exhibits a decided difference from that for the *ɔ* of "saw," although both vowels are generally considered to be the same. The *ɔ* of "caught" shows a quick and strong increase in amplitude followed by a rather sudden decrease. Its pitch is approximately constant. The initial strong vibration of a group is followed by very much weaker vibrations; the cord action resembles that in *a* rather than in the *ɔ* of "saw." In the last few groups there is a marked change as the *ɔ* alters to *t*.

The cord tone rises from a period of 0.0074^s (frequency, 135) to one of 0.0064^s (frequency, 156) but falls again in the last few periods. The lower cavity tone seems to have a period of about 0.0024^s (frequency, 417). Other tones of higher pitch are present.

In the *e* of "said" the vocal action is seen to differ essentially from that in *a* or *ɔ* and to resemble somewhat that in *i*. There is much less indication of the explosive character of the cord tone. There are three cavity vibrations to each group. The pitch of the cord tone is nearly constant at 0.0072^s period (frequency, 139); the lower cavity tone has a period of 0.0024^s (frequency, 417). There are minor fluctuations in the curve that indicate higher cavity tones. The amplitude increases steadily until the vowel is ended rather abruptly by the change to *d*.

IV. Studies of the Jefferson curves.

Joseph Jefferson[1] was born at Philadelphia in 1829. He grew up in the midst of theatrical surroundings. He was brought on the stage at the age of four, and showed unusual imitative ability. His most famous part is that of Rip Van Winkle in the play of that name.

Joseph Jefferson's great-grandfather, Joseph Jefferson, was an English actor of prominence. His grandfather, Joseph Jefferson, was born at Plymouth, England: as a lad he acted upon the stage; at about twenty years of age he migrated to America where he achieved dis-

[1] Jefferson, The Autobiography of Joseph Jefferson, New York 1889.
Winter, The Jeffersons, Boston 1881.

tinction as an actor; he married the American-born daughter of an emigrant Scotch merchant. His father was Joseph JEFFERSON, an American actor and painter.

His maternal grandfather and grandmother were French. On a journey from France to San Domingo a daughter was born to them in New York City. They resided in San Domingo till 1803, after which they lived in Charleston, South Carolina. The daughter, Cornelia Frances Thomas, won an excellent rank in Charleston as an actress and a singer. Her first husband was the Irish comedian, Thomas Burke. Her second was Joseph JEFFERSON, father of the speaker of these records.

Owing to his mixed ancestry, to the constant wanderings of his parents and himself, and to the actor's tendency toward freedom from dialectal peculiarities, JEFFERSON'S speech is typically American in every sense that can be given to the term.

A gramophone disc, numbered 698 Z, containing *Rip Van Winkle's Toast* spoken by Joseph JEFFERSON was traced off by the machine shown in Fig. 1. The words on the plate were "Come, Rip, what do you say to a glass? What do I say to a glass? Huh, now what do I generally say to a glass? I say it is a fine thing—when there's plenty in it. Ha! So. You had it ten years ago, eh? Ah. That's fine schnapps. I wouldn't keep it as long as that, would I? Huh, huh. Well, here's your good health, and your family's; and may they all live long and prosper. Ah." The complete tracing is reproduced in Plates III to XI. Each group of words on these plates refers to the following portion of the curve. The figures show the number of millimeters of straight line cut out of the record.

A. The melody of *Rip Van Winkle's Toast.*

After a general analysis[1] of the record the lengths of the successive groups of vibrations were carefully measured in tenths of a millimeter. The lengths were turned into time by the equation $1^{mm} = 0.0007^{s}$; the results were the periods of the cord tone. The reciprocals of the periods gave the frequencies. For example, a group of vibrations corresponding to one explosion from the cords measured 0.0038^{mm}; multiplied by 0.0007 this gave a period of 0.0061^{s}, and $1 \div 0.0061$ gave a frequency of 167. The curve of frequency (pitch, or melody) was plotted by supposing the original speech curve, as in Plates III to XI, to be laid along a horizontal axis, and erecting above the beginning of each group an ordinate proportional to the frequency. In this way the curve of frequency for the entire set of plates was plotted on a strip (over 17

[1] The methods of analysis are described in SCRIPTURE, Elements of Experimental Phonetics, Ch. V, New York 1902.

meters long) of millimeter paper. This was then divided into convenient pieces and made into two blocks (Plates XII and XIII) with a reduction to one-fifth. The horizontal scale of time in the melody plates is thus $1^{mm} = 0.0035^{s}$, or *one-fifth* that in the original speech plates. The dots of the plot were joined by straight lines. This gives the results accurately; but a more truthful representation of the melody-effect would be made by a curve running smoothly through the dots (p. 58).

The vertical scales indicate frequency, or the number of vibrations a second. Each group of words refers to a portion of the melody-curve extending from its beginning to a group of large figures on the horizontal line; during each portion the horizontal line remains unbroken. The large figures indicate, as in Plates III to XI, the portions of straight line in the original tracings that were omitted in preparing the plates; they may be turned into time by the original equation of $1^{mm} = 0.0007^{s}$.

The interruptions in the melody-curve indicate surds, or very weak sonants, or pauses.

The curve in Plates XII and XIII shows a very low and even melody of speech that is varied at times for emotional expression. In general each sentence begins low, rises gradually, and then falls; but variations occur. The changes in the tone are usually continuous.

"Come Rip" shows a rise at the end, which is a common inflection for a cheerful, animated invitation. "What do you say to a glass?" shows a low vowel, then a rise to the *u* of "you"; this *u*, however, begins to fall just before the following word. "Say" is of high pitch, as is frequently the case for the verb of a question; the fall at the end of "you" may have been a kind of preparation by contrast for the high pitch of "say." The highest pitch for the phrase is found in "glass"; it is even higher than in "say," probably because of the greater emphasis given to the word "glass." The pitch falls toward the end of *æ* in "glass"; such a fall is usual in a sentence beginning with an interrogative word or phrase that is not especially emphatic. These words were spoken by JEFFERSON as introductory to the *Toast* itself. The invitation is followed by a long pause of 2.86^{s} before the reply comes.

The toast begins with a repetition of the question of invitation. It is spoken in a rather soft manner, as appears not only to the ear but also in the small amplitude of the waves in Plate IV. The pitch curve is fairly level, with some rise at the end instead of a fall. This rise is the usual ending of a repeated interrogative sentence. The general pitch is lower than that of the invitation. A pause of 0.41^{s} follows.

The exclamation "Huh" is a kind of chuckle. It is of a very high

pitch but small intensity and short duration. It is followed by a pause of 1.05^s.

"Now what do I generally say to a glass?" shows a very even rise and a very gradual fall; its general pitch is low. It is a kind of bantering statement. The long pause of 2.16^s seems to express a simulated expectation of a reply.

"I say it is a fine thing" is a decided statement with emphasis on "fine thing." It has the usual circumflex form as far as "a." If the sentence had been completed with no further emphasis, the pitch would probably have continued to fall. The rise in pitch for the specially emphatic "fine thing" adds an accessory circumflex. The pause of 1.78^s and the fall in pitch lead the hearer to suppose the sentence finished.

"When there's plenty in it" is muttered as a joke. Its pitch is not lower than usual. The emphasis "plenty in it" gives a higher pitch to the latter portion. The whole statement has the usual circumflex form. The long pause 2.90^s is presumably occupied by the first sip of the toast.

The soft exclamation of satisfaction "ha" has a falling pitch. It is followed by a pause of 1.79^s. The "so" expresses deep satisfaction. It begins moderately high and falls steadily in pitch. To the ear it has a peculiar rattle of a low pitch as if some particles of liquor had lodged on the edge of the epiglottis, as is sometimes the case after drinking. This peculiar effect shows itself in the alternately louder and weaker character of the groups of vibrations as seen in Plate VII. Such a curve could be produced by the cord explosions striking against a mass of liquid that would vibrate readily at a submultiple of the cord period: the portion of liquid would rise and fall, weakening the cord tone on alternate periods. It is probable that when speaking into the gramophone recorder JEFFERSON produced this effect by some muscular adjustment (epiglottis, ventricular bands) and not by an actual sip of liquid. "So" is followed by a pause of 2.00^s.

"You had it ten years ago, eh?" is spoken as a continuous sentence; there is complete fusion of the vowels at the end. The first part rises rapidly to a high pitch. The circumflex form is marked, the fall beginning in the *o* of "ago." The "eh" has a circumflex form joined to the *o* curve. In spite of the complete fusion of these vowels we may perhaps consider "eh" as a stressed tag with a pitch-curve of its own. The sound indicated here by "eh" begins with a very weak breathing and seems slightly nasalized. The long pause of 2.45^s indicates perhaps the time of another sip.

"Ah" is an expression of satisfaction; it appears to the ear much lower and smoother than the "ha." The following pause is very short, 0.13^s.

"That's fine schnapps" is not an emphatic statement but expresses a decided conviction after a satisfactory trial. It shows the usual initial rise for a declarative sentence, but instead of falling at the end, it rises slightly. This peculiar rise seems to express conviction after a doubt. The figures 333^{mm} after "that's" indicate the portion of tracing (*tsf*) left out in the original record, and not a pause. The sentence is followed by a pause of 1.92^s.

"I wouldn't keep it as long as that" has the usual circumflex form; it is followed by a brief pause of 0.29^s.

"Would I" is used to turn the declaration into question. It is very brief. A short pause, 0.25^s, follows.

The very brief and faint chuckle "Huh, huh" is followed by a pause of 1.20^s.

The introductory "Well"—presumably spoken as the glass is lifted —rises steadily to a high pitch. It is followed by a long pause of 3.43^s.

"Here's your good health" rises steadily to a very high pitch. The speaker makes a rather long pause, 0.94^s, perhaps for emphasis. He then completes the thought in his mind by "and your family's." This tag-phrase has, however, somewhat the character of a separate sentence; its pitch-curve is circumflex. It is followed by a pause of 1.54^s.

The invocation "and may they all live long and prosper" appears to have the solemn steady intonation of a somewhat religious utterance. The pitch-curve shows great evenness; there is a rise at the beginning and a fall at the end. The fall appears during the first part of "prosper"; during the last part the cords have so relaxed that they produce only a few rather irregular vibrations (Plate XI); this last syllable appears to the ear almost as a surd or whispered one. It is followed by a pause of 1.74^s, during which the toast is presumably drunk.

The "ah" is a low, soft exclamation of gustatory satisfaction after the toast. The peculiar rattle is heard as in "so" above; the same alternation in the character of the groups of cord vibrations appears. The pitch-curve shows a steady fall. The last vibrations are of a very low pitch; they appear clearly in the tracing but they are probably too low for the ear to catch.

B. Duration of sounds in *Rip Van Winkle's Toast.*

Measurements of the lengths of the speech sounds in the JEFFERSON records were made by an assistant under my guidance. The complete-

TABLE.

k	?
ə	0.22[8]
m	.11
ɹ	.15
i	.21
p ‿ *hw*	.33
ə	.19
d	.06
ə j u	.66
s	.24
e	.24
t	.10
u ə	.31
g l	.14
æ	.57
s ‿ *hw*	3.94
ə	.19
d u a i	.43
s	.16
e	.21
t	.10
u ə	.30
g l	.10
a	.45
s ‿ *h*	.46
ə	.16
‿ *n*	1.28
au	0.22
hw	.04

ə	.22
d	.02
u a i j e n ɹ ə l i	1.33
‿	.14
[illegible]	.22
t	.07
u ə	.25
g	.02
l	.11
æ	.35
i ‿	2.23
a i	.25
s	.11
[illegible] *i*	.25
t̬	.05
i	.16
z	.04
ə	.13
f	.10
a i	.33
n θ	.10
i	.29
ŋ	.10
‿ *hw*	1.90
e	.16
n ð	.13
ə	.14

z p	.18
l	.03
e n t	.22
i i n i	.45
t ‿ *h*	3.07
a	.32
‿ *z*	1.86
o	.49
‿	2.06
j u h ɹ	.39
d	.06
i	.11
u	.19
e n j i ə	.70
z	.02
ə	.08
ɹ	.09
o [illegible] (?)	.80
‿	2.52
a	.52
‿ *ð*	.19
ɹ	.22
t s f	.26
a i	.23

n	.14
š	.12
n	.09
æ	.24
p i ‿	1.97
a i	.30
w	.07
u	.11
d	.03
n	.16
t k	.11
i	.13
p̂ i t	.06
ɹ	.11
z	.06
l	.24
ə	.34
ŋ	.07
ə z ð e	.93
t ‿ *w u l*	.32(?)
a i	.09(?)
‿	.30
[illegible]	.13
‿ *w*	1.14
e	.32
l ‿ *h*	3.56
i ə	.29

z	.09
j u	.24
g	.02
u	.16
d h	.03
ɹ	.30
l	.07
θ ‿	.97
ə	.16
n	.16
j uə	.14
f	.12
æ	.23
m	.07
ə	.06
l	.08
i	.13
z ‿	1.64
æ	.01
n m	.12
e	.16
ð	.05
e	.31
ɔ ll i	1.76
v	.10
l	.18
ɔ	.62
ŋ ə	.34
n ‿ *p r*	.10
a	.30
s	?
p	?
ə	?

ness of the fusion of sounds in connected speech[1] made it impossible to assign any very definite limits to most of the sounds. When a sound was next to a pause or a surd, its limit was placed at the extreme vibration. Thus the first vibration of *ə* in "come" (Plate III, line 1) and the last distinct one of *i* (line 4) gave fairly definite limits. Yet the curve shows quite clearly that the *i*-vibrations began to be weakened by closure for the *p* somewhere about 60^{mm} from the right end of line 4; faint vibrations can, however, be detected at about 15^{mm} from the end; thus, even in a case like this, it is impossible to mark off the limits of *i*, *i-p* glide, and *p*. In other cases there is no possibility of assigning any limits, because the sounds are fused into gradually changing ones; thus in line 13 the *u* of "to" changes to *ə* "a," but the change is a gradual one beginning far back in the *u* and extending throughout the *ə*. In fact, there are *not* two sounds *u* and *ə* united by a glide: there is a changing sound which at some one instant may be an *u* and at a later one may be an *ə*, and which to the ear (trained to various associations) gives an impression resembling a sequence of *u* and *ə*. In spite of these facts I venture to give figures for the duration of sounds in these JEFFERSON records in order to furnish some approximate data; the figures are subject to the limitations just explained: where I have been utterly unable to decide on a limit I have indicated the fusion by a brace in the Table.

The phonetic notation is used in the Table merely to indicate the sounds in order to aid in marking off their duration; it is not intended as an accurate phonetic analysis. For example, the use of *ə* for the short vowel in "what" does not necessarily mean that the sound is identical with the *ə* in "come"; to the ear the brief vowel in this case seems related to *a*, *ɔ*, and *ə*, but it is hardly possible to decide on the degrees of likeness. The symbol t_* indicates a sonant *t*.

V. STUDIES OF VIBRATING SPRINGS.

According to the HELMHOLTZ theory[2] a vowel is produced by a cord vibration of the nature of a sinusoid or a harmonic series of sinusoids acting on a resonating cavity or on a set of such cavities. The method of manufacturing a vowel by synthesis of tones would, if this theory were

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[1] SCRIPTURE, Elements of Experimental Phonetics, 452, New York 1902.

[2] HELMHOLTZ, *Über d. Vokale*, Arch. f. d. holl. Beitr. z. Natur- u. Heilk., 1857 I 354.

HELMHOLTZ, *Über d. Klangfarbe d. Vokale*, Gel. Anz. d. k. bayr. Akad. d. Wiss., 1859 537; also in Ann. d. Phys. u. Chem., 1859 CVIII 280, and in Ges. wiss. Abhandl., I 395. 397, Leipzig 1882.

HELMHOLTZ, Die Lehre v. d. Tonempfindungen, 5. Aufl., 168, Braunschweig 1896.

true, consist in adding tuning-fork tones of different pitches and intensities. This was attempted in HELMHOLTZ's vowel apparatus.[1] The method of manufacturing a curve like a vowel curve would, if the theory were true, lie in adding sinusoid vibrations of different periods and amplitudes. This was attempted with the harmonic curve adder of PREECE and STROH.[2] HELMHOLTZ's synthesis succeeded only for *u* and *o*; it failed for all other vowels. PREECE and STROH produced curves that at best only distantly resembled vowel curves. The theory thus failed in both cases.

According to the WILLIS-HERMANN theory[3] the cords emit puffs of greater or less sharpness, which act on the vocal cavities like sharp blows. I have attempted to construct a vowel machine on this principle; the

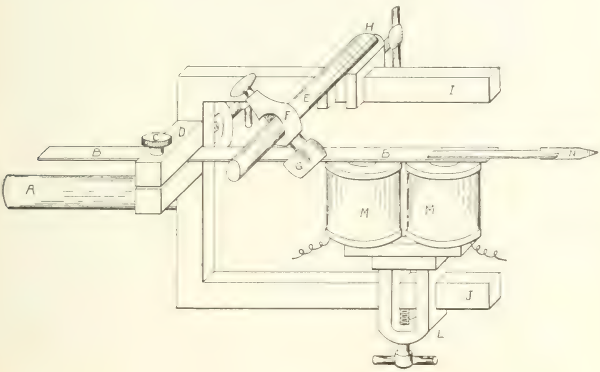

FIG. 10

account will be given later. I have also attempted to manufacture vowel curves by vibrating springs moved by sharp blows; on the present occasion I will describe the apparatus and give some of the earlier results.

The steel spring *B* (Fig. 10), clamped tightly in a small vise *D* on

[1] HELMHOLTZ, Die Lehre v. d. Tonempfindungen, 5. Aufl., 200, Braunschweig 1896.

[2] PREECE AND STROH, *Studies in acoustics, I. On the synthetic examination of vowel sounds*, Proc. Roy. Soc. Lond., 1879 XXVIII 358.

[3] WILLIS, *On vowel sounds and on reed-organ pipes*, Trans. Camb. Phil. Soc., 1830 III 231; also in Ann. d. Phys. u. Chem., 1832 XXIV 397.

HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1890 LVIII 274.

HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LXXIV 380, 381.

the frame IJ, bears at its end a recording point N of thin steel ribbon. The frame also carries an adjustable electro-magnet M clamped in place by L and a felt damper G adjusted as desired by the clamps F and H with their rod E. The rod A is placed in a supporting standard (Fig. 11),

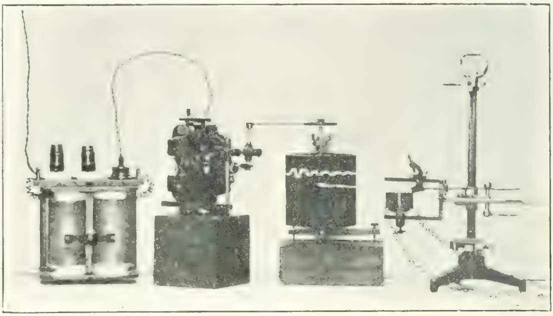

FIG. 11.

which is so adjusted that the recording point rests against the surface of a smoked drum.

The drum is rotated by a small electric motor whose speed is regulated by an appropriate resistance; Fig. 11 shows both a lamp resistance for large changes in speed and an adjustable wire resistance for smaller changes.

A blow on the spring B causes it to draw a sinusoidal line on the drum; the waves, however, slowly decrease in amplitude, owing to loss of energy by friction. A quicker decrease, due to additional damping, can be ob-

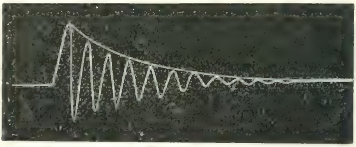

FIG. 12.

tained by placing the surface of the felt damper G more or less tightly against its edge. A curve of vibrations dying away by friction due to damping is shown in Fig. 12; it was made by the damped spring struck by a blow.

When a material point is displaced from the position of equilibrium to which it is attracted by a force that increases directly as its displacement,

and then released, its vibration can be expressed with close approximation by

$$y = a \cdot e^{-kt} \cdot \sin 2\pi \frac{t}{T},$$

where y is the displacement of the point at the moment t, a the amplitude, e the constant 2.71828, k a factor depending on the relation between the mass of the point and the amount of the friction, and T the period under the given circumstances. The amplitude a is subjected to a steady decrease by the divisor e^{kt}, for in the expression $a \cdot e^{-kt} = a/e^{kt}$ the amplitude will have its greatest value only when $k = 0$ or when there is no friction. Any friction will give a positive value to k and this will reduce the value of a. When there is friction the value of e^{kt} will increase proportionately as time elapses; thus a will be steadily reduced. The equation is illustrated by the curve in Fig. 12; the line drawn along the summits of the waves is the curve of amplitude $a \cdot e^{-kt}$.

A vibratory body may receive a series of impulses. The results of different natural periods of the vibratory point, of frictional factors, of

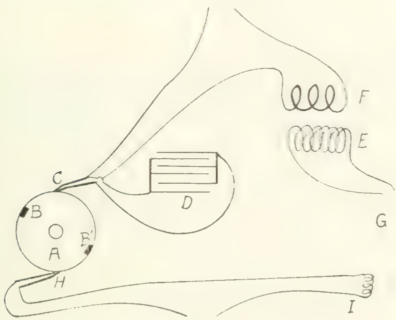

FIG. 13.

various strengths of impulse and of different intervals of repetition, can be studied by means of the vibrating spring. A series of impulses may be imparted to the spring B (Fig. 10) by brief electric currents sent through the magnet M. In a study of the action of such impulses on a spring these impulses were obtained and recorded in the following way. A hard rubber contact wheel A (Fig. 13) carried on its rim two pieces of metal BB'. A pair of copper brushes H bearing against the rim were the poles of a circuit through the magnet M (Fig. 10), indicated by I in

Fig. 13. As B or B' passed across H, it closed the circuit and sent a magnetic impulse to the spring. This had the effect of a sharp blow. The strength of the blow could be readily adjusted by varying the current or displacing the magnet M. As it was desirable to have an indication of the exact moment at which the impulse was sent to the spring, a spark coil was made to register directly on the line drawn by the vibrating point. A pair of copper brushes C formed the poles of a circuit through the primary coil F of a spark coil, whose secondary coil E was connected by the wires G to the metallic spring and the base of the recording drum. A condenser D was connected around the break at C. Whenever a metal piece B or B' passed under the brushes C, the circuit was closed. With an appropriate adjustment of the current, a spark passed from the recording point through the paper to the drum, removing the smoke and making a white dot when the circuit was closed and also when it was broken. The two pairs of brushes were so adjusted that the sparks registered exactly the moments at which the impulses were sent through the magnet and those at which they ceased.

A record of an experiment in which the contact wheel was revolved with steadily increasing rapidity is reproduced in Fig. 14. The waves were drawn by the point N (Fig. 10); the pairs of dots marked the

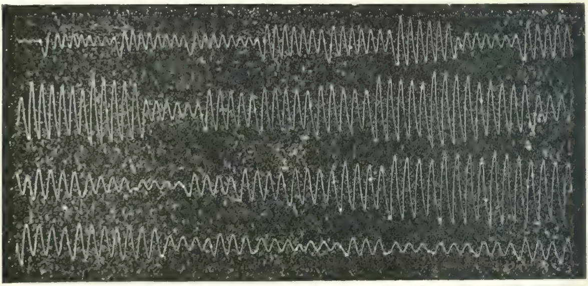

FIG. 14.

beginning and end of each impulse. The figure shows that each impulse started a vibration which died away by friction. If one impulse followed the preceding one before the vibration was entirely gone, its effect was increased or diminished according as the phase of movement in which it occurred was the same as or opposed to the movement started by the impulse. When the impulses occurred quite close together and at exactly the right phases, the summation of effects made the vibra-

tions very strong. In all such cases an increase occurred in amplitude whenever the period τ of the impulses became a multiple of the natural period T of the spring. In all cases the spring vibrated with the period T; only the amplitude was affected by the vibrations of τ.

The condition of equal lengths of impulse could not be illustrated with the arrangement just described, as the contacts through B and B' (Fig. 13) lasted a constant fraction of a revolution and the length of the impulse decreased proportionately as the speed of revolution increased. The impulses were weaker as they came faster. Nevertheless the increase in amplitude whenever τ was a multiple of T appears strikingly in Fig. 14. This increase in amplitude for harmonic relations (that is, according to the simple ratios 1 : 2 : 3 : etc.) between the natural period and that of the impressed force is known as 'resonance.'

A synthesis of two frictional sinusoids may be accomplished by the arrangement shown in Fig. 15. The spring B is the spring B of Fig. 10. Upon it there is placed the slide V carrying the spring U and another slide R with the electro-magnet S. The movement of B is recorded on a smoked drum by the point N, that of U by the point T. The magnet M of the spring B (Fig. 10) and S of the spring U (Fig. 15) are connected with the contact wheel A (Fig. 13). When the current passes through M alone, both points N and T draw the curve of vibration for B as in Fig. 14. When sent through S alone, the point T draws the curve of vibration of U. In both cases the vibration is a free frictional sinusoid. When the current is sent through both M and S, the point T draws the curve of the sum of the vibrations of B and U. The relations

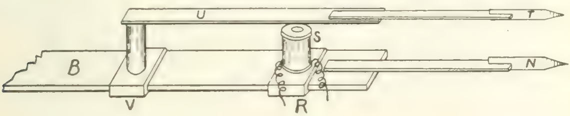

FIG. 15.

of period may be altered by changing the lengths of B and V, those of amplitude by shifting the magnets, those of damping by adjusting the dampers. When the curve drawn by T is like that found in a speech curve, it can be assumed that the speech curve is the result of two vibratory movements simultaneously aroused by a sudden blow, which have relations of pitch, amplitude and damping like those in the springs. The sudden blow is the puff from the cords heard in the cord tone and the two free vibrations are those of the vocal resonance cavities. Tables of typical combinations would be useful. A third sinusoid might be added

by placing another spring and magnet on U in the same way as U and S on B. Work on these problems is now in progress; tables of curves may be expected at some future date.

VI. Studies of breathing.

Records were obtained by an earlier form of the Marey pneumograph. This consisted of a rubber tube held distended by a coiled

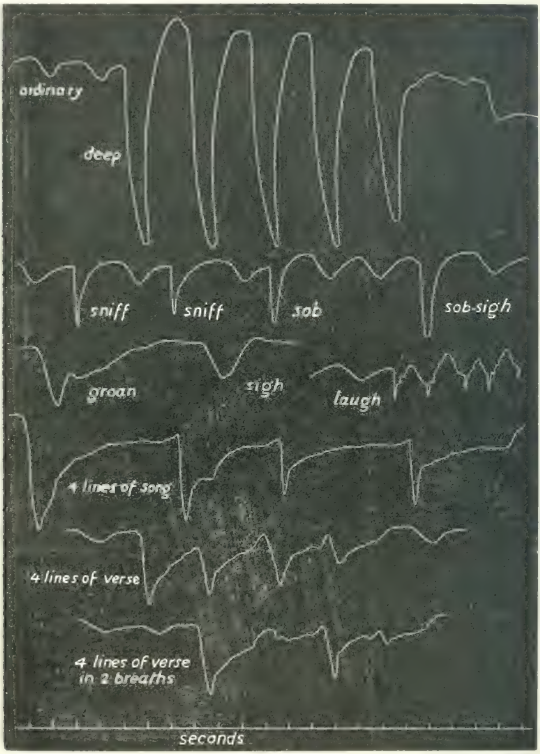

Fig. 16.

spring; the ends were closed; one of them communicated with a Marey tambour writing on a smoked drum. The set of records shown in Fig. 16 was made on myself.

Ordinary breaths followed by several deep ones are shown in the topmost records; it will be noticed that the movements are very small after the blood has been refreshed by deep breathing. A record of ordinary breathing interrupted by sniffing, sobs and a sigh-like sob are shown in the second record; the inspirations are very sudden. The curves for a groan and a sigh are also shown in the third record; the inspirations are not sudden, and the expirations are more gradual than in the sigh, the groan showing a specially long and irregular expiration. All these sobs, groans and sighs were produced premeditatedly. A series of premeditated laughs is also shown. Each laugh consisted of 'ho-ho-ho-ho' with falling pitch; the laugh occupied the expiration-half of each curve. The record marked '4 lines of song' shows the breath expenditure during the singing of

> "Way down upon the Swanee River,
> Far, far from home;
> Oh, darkies, how my heart does quiver,
> Far from the old folks at home."

The expiration of the breath not used during each line appears clearly each time at the end. The next to last record shows the use of the breath in speaking the verses

> "The Cities are full of pride,
> Challenging each to each;
> This from her mountain-side,
> That from her burthened beach" (KIPLING).

The inspiration occurred just before the beginning of each line. The

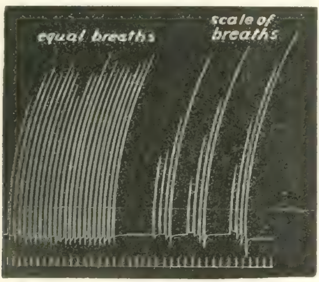

FIG. 17.

last record shows the breath-expenditure when the stanza was spoken more rapidly: one deep inspiration with a slight accession afterwards is

made to do for each pair of lines. The discharge of the air not used in speaking is indicated by the sudden rise at the end of each line. Both records were made with no intentional distribution of the inspirations. The time-line with seconds is given for all these records at the bottom.

Records of the air-pressure at the mouth were made by putting the end of a rubber tube loosely in the corner of the mouth and attaching the

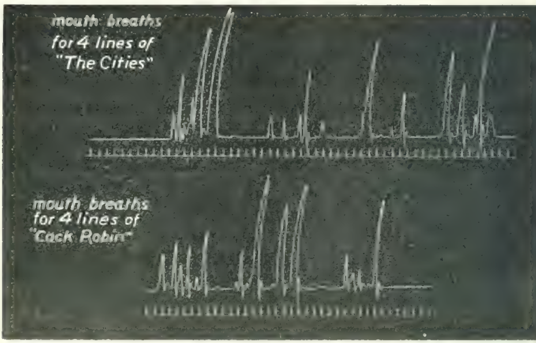

Fig. 18.

other end to a tambour recording on a smoked drum. Records of pressure intended to be equal and of some intended to be in the relations of 1 : 2 : 3 : 4 in intensity are shown in Fig. 17. Records of the variations in mouth-pressure during the recitation of four lines of Kipling's "The Cities," and of four lines of the nursery rhyme "Cock Robin," are shown in Fig. 18.

EXPERIMENTS ON MOTOR EDUCATION

BY

W. SMYTHE JOHNSON.

This series of experiments was instituted to determine the effects of light gymnastic exercise on quickness of voluntary movements and the development of the power of concentration of attention. It has been generally accepted that the direct effect of physical training is the development of strong and sinewy muscles and that mental quickening is only an indirect result of such training. The experience of the Elmira Reformatory showed that physical training has an educative value. By a graduated system in physical culture, the inmates who constantly failed in the School of Letters, in the Trades School and in deportment, were enabled to return and maintain their places in the regular institutional life.[1]

The experiments[2] carried out in this laboratory, during the academic years 1898–1900, by W. W. DAVIS and myself, showed conclusively that the effect of practice in speed and accuracy of voluntary movements was not limited to the member which was exercised, that the gain was principally conditioned by the power to concentrate attention, that the approximate highest rate of speed and the least variation in consecutive movements would be reached in from six to ten days with short practice each day, that the greatest gains were made during the first three days. It was also observed that those who developed most rapidly were those who took more or less gymnastic exercise.

In the following experiments the subject was seated in a quiet room, separate from the experimenting room. He was required to react to a sound coming from a click in a telephone. When all was ready for the record, a signal was sent into the quiet room by means of a telegraph-sounder. The subject understood that in one to three seconds after this signal he would hear a click in the telephone, to which he was to respond as quickly as possible by pressing on a telegraph key. Two degrees of intensity were selected, the subject knowing which would be used. The time was recorded on a pendulum chronoscope.

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[1] SCRIPTURE, *Cross-education*, Pop. Sci. Monthly, 1900 LVI 589.

[2] DAVIS, *Researches in cross-education*, Stud. Yale Psych. Lab., 1898 VI 6.

JOHNSON, *Researches in practice and habit*, Stud. Yale Psych. Lab., 1898 VI 51.

First series of experiments.

The subjects were students in the Graduate School of Yale University, with ages ranging from 23 to 26 years. Subjects *A* and *D* were of a phlegmatic temperament; *B* and *C*, of a nervous temperament. All were in good health; they had not previously been taking any systematic exercise.

For each of the four subjects included in this test, ten records with the loud and weak sounds were taken at each sitting. After three sittings on successive days, they were then given pairs of six-pound dumbbells, and requested to practice with them for a few minutes during different intervals of the day; more especially just before retiring at night and on rising in the morning. The average daily practice for each of the four subjects was 45 minutes. They were not informed of the object of the practice with the dumbbells. After they had practiced for approximately two weeks, they were then subjected to another series of three tests in reacting exactly like the first.

The results (Table I) show a shortening of the average reaction-time ranging from 17^σ to 84^σ. In order to make clear the meaning of the figures given in Table I, I will explain those for subject *A* in detail. The figures 285^σ, 262^σ, and 221^σ are each the average of ten records, and denote the average time that it required to respond to the signal at each successive sitting. The average time of the first three sittings, 242^σ, is the average of the sum total of the records taken during the first three sittings. The average probable error was derived in like manner.

The average of the records taken after the practice with the dumbbells was 192^σ, thus showing a gain of 50^σ. But the gain for the weak sound shows more favorable results still, namely, 84^σ. The results for *A* clearly demonstrate the influence of systematic muscular training on the development of a sensitiveness to auditory stimuli. This subject being a little hard of hearing, his reactions to the weak sound were extremely slow previous to the practice with the dumbbells; but after the practice his responses to the weak sound were almost as quick as for the loud sound.

The table also shows regularly a decrease in the probable error.

It is evident that the regularity with which one responds to a given signal will depend upon how closely the attention is fixed on the action to be performed at each successive response. Hence the probable error of the ten records made at each sitting is a clear index of the subject's power to hold his attention during the performance of ten successive responses to a signal. If he allows the attention to be distracted by wandering thoughts, then the probable error will be increased.

The probable error, however, may depend on any one or more of the following causes: (a) fluctuations of attention, (b) actual increase in speed during the progress of an experiment, (c) fatigue of the nervous system. That the probable error given in Table I for all the subjects is due to (a), or lack of power to concentrate attention only, is evident for two reasons: (1) the time required for the experiment was too short to fatigue the system, and (2) the individual records show that there was no tendency to either increase or decrease of the reaction-time during the progress of an experiment.

TABLE I.

Subject.	Date.		*Reaction to loud sound.* Daily ave.	Ave. of three days.	Decr. after interval.	Daily p. e.	P. e. of three days.	Decr. after interval.	*Reaction to weak sound.* Daily ave.	Ave. of three days.	Decr. after interval.	Daily p. e.	P. e. of three days.	Decr. after interval
A	Jan.	22	285			37			320			44		
	"	23	262			43			308			45		
	"	25	221	242		31	33		257	282		40	42	
	Feb.	8	184			13			185			18		
	"	9	183			22			193			27		
	"	10	209	192	50	23	22	11	217	198	84	20	26	16
B	Jan.	20	210			19			215			24		
	"	22	216			24			248			19		
	"	23	251	225		29	25		243	235		30	26	
	Feb.	8	213			11			222			20		
	"	9	206			17			212			7		
	"	10	207	208	17	17	11	14	200	211	24	9	15	11
C	Jan.	22	278			63			234			29		
	"	23	247			25			225			16		
	"	25	180	234		15	48		173	204		10	31	
	Feb.	8	183			8			183			10		
	"	9	188			9			179			7		
	"	10	193	188	46	9	9	39	192	185	19	6	9	22
D	Jan.	18	289			45			302			45		
	"	19	218			25			227			51		
	"	20	189	232		29	29		191	240		29	44	
	Feb.	8	224			21			231			31		
	"	9	192			17			221			21		
	"	10	196	207	25	18	20	9	214	222	18	22	24	20

The unit of measurement is $1^{\sigma} = 0.001^{s}$.

Each figure under *Daily ave.* is the average of 10 measurements.

Each figure under *Ave. of three days* is the average of 30 measurements.

P. e., probable error.

Unlike such voluntary movements as the continuous tapping on a telegraph key, where the regularity of the tapping is proportionate to the development of subconscious control, in this form of action the reverse is true. It depends upon the power to hold the attention on the expected stimulus and the action to be performed.

A response to a sensory stimulus was the simplest form of action to be devised to test the influence of physical culture on the above-mentioned mental attributes, and was withal the most accurate test. The beneficial effects of the dumbbell exercise on the development of the power of concentration of the attention are clearly shown in the large decrease of the probable error in the experiments made after the practice with the dumbbells, varying from 9^σ to 39^σ. For instance, the average probable error for *C* previous to dumbbell practice was 48^σ and afterwards only 9^σ, thus showing a net gain of 39^σ in regularity of response to the sound in the telephone. This means, I believe, that the power of holding the attention was better developed in the latter part of the series of experiments.

Second series of experiments.

The subjects included in this series were juniors in the Academical Department. They were, at the time of the taking of the records, studying the question of movements in a course in experimental psychology. The four subjects were *E* (27 years of age, phlegmatic temperament, fair health, no gymnastic exercise), *F* (30 years, nervous, poor health, no exercise), *G* (24 years, phlegmatic, good health, no exercise) and *H* (22 years, nervous, good health, some exercise).

The subjects, instead of practicing with the dumbbells, were requested to spend some time each day in picturing as vividly as possible the processes involved in responding to the sensory stimulus.

The same apparatus was used and the conditions of experimentation were the same in this series as in the first. The subjects were each given three sittings on successive days. At each sitting ten records were made with the loud sound and ten with the weak one. After these records were taken the object of the experiment was explained to them, and they were asked to think about the matter for two weeks, after which they would be called in for another series of records. The experimenter made it convenient to speak with them on several different occasions during the interval between the taking of the records. Moreover, they were given some reading matter on the subject of reaction-time. The average time spent in reflection and reading on the subject was 35 minutes each day, varying, however, with each of the subjects.

The results given in Table II show considerable decrease in the re-

TABLE II.

Subject.	Date.	Reaction to loud sound. Daily ave.	Ave. of three days.	Decr. after interval.	Daily p. e.	P. e. of three days.	Decr. after interval.	Reaction to weak sound. Daily ave.	Ave. of three days.	Decr. after interval.	Daily p. e.	P. e. of three days.	Decr. after interval.
E	Feb. 20	252			26			248			15		
	" 21	256			15			271			22		
	" 22	224	244		14	20		224	248		16	21	
	March 12	197			11			196			12		
	" 13	190			11			183			7		
	" 14	200	195	49	11	11	19	218	199	49	12	13	8
F	Feb. 20	256			36			238			18		
	" 21	245			26			252			15		
	" 22	225	242		22	31		200	230		18	22	
	March 13	195			19			212			17		
	" 14	206			17			219			18		
	" 15	202	201	41	17	17	4	215	216	14	17	17	5
G	Feb. 20	286			33			262			30		
	" 21	260			29			254			14		
	" 22	236	254		19	23		248	255		16	21	
	March 13	240			20			228			14		
	" 15	210			22			218			15		
	" 16	214	221	33	31	23	0	218	221	34	23	17	4
H	Feb. 20	337			26			358			35		
	" 22	320			27			310			29		
	" 23	246	298		16	35		220	296		13	45	
	March 13	195			25			215			18		
	" 14	217			27			198			28		
	" 15	216	209	89	21	24	11	214	209	87	15	21	24

The unit of measurement is $1\sigma = 0.001^s$.

Each figure under *Daily ave.* is the average of 10 measurements.

Each figure under *Ave. of three days* is the average of 30 measurements.

P. e., probable error.

action-time, even exceeding in amount that given in Table I. But the reaction-time for the subjects included in Table II was much slower at the beginning of the series than for those included in Table I. Even though the gain in reaction-time was greater for Series II, it does not signify that the proportionate gain was greater, for the reaction-times at first were much longer. That the gain in reaction-time for Series II was largely due to a special effort is shown in the comparatively small decrease of the probable error.

Subjects *E* and *H* each took much interest in the work; and from the remarks made, I am persuaded that the large decrease in their reac-

tion-time was largely due to the special effort put forth. In fact, all four of the subjects included in this series had the advantage of knowing the purpose of the test; this no doubt had its influence in calling forth a greater effort on their part than for those included in Series I and III. This is especially true of subjects *E* and *H*. But with even this additional incentive, when we take them by classes, we find that those of Series I were not only quicker than those of Series II in responding to the signal after the interruption of two weeks, but the probable error was also less.

Third series of experiments.

The conditions of experimenting were the same as in Series I and II, except that during the interval of two weeks elapsing between the third and fourth sittings nothing was required of them. In fact, they did not know that they would be called in for another test after the first series were over.

The subjects were students in the Graduate School: *I* (28 years of

TABLE III.

		Reaction to loud sound.						*Reaction to weak sound.*					
Subject.	Date.	Daily ave.	Ave. of three days.	Decr. after interval.	Daily p. e.	P. e. of three days.	Decr. after interval.	Daily ave.	Ave. of three days.	Decr. after interval.	Daily p. e.	P. e. of three days.	Decr. after interval.
I	Feb. 21	230			34			218			22		
	" 22	190			12			208			18		
	" 23	213	211		10	23		209	212		18	19	
	March 13	193			8			202			20		
	" 14	229			24			248			15		
	" 15	213	211	0	13	19	4	224	224	—12	15	20	—1
J	March 5	331			31			363			48		
	" 6	256			19			258			19		
	" 8	223	270		13	29		242	270		22	31	
	" 14	212			11			238			18		
	" 15	244			19			225			13		
	" 16	234	230	40	27	19	10	232	232	38	27	19	12
K	Feb. 21	293			32			318			34		
	" 22	293			38			266			28		
	" 23	258	281		36	36		259	281		34	35	
	March 13	229			27			243			21		
	" 14	234			21			229			21		
	" 15	211	225	56	23	23	13	207	226	55	12	21	14

The unit of measurement is $1^{\sigma} = 0.001^{s}$.

Each figure under *Daily ave.* is the average of 10 measurements.

Each figure under *Ave. of three days* is the average of 30 measurements.

P. e., probable error.

age, phlegmatic temperament, good health, athlete), *J* (36 years, phlegmatic, good health, outdoor exercise), *K* (25 years, nervous, good health, no exercise).

The results of this series of experiments are given in Table III. They show comparatively small gains both in reaction-time and in decrease of the probable error. The records of subject *I* show a reverse of what might be expected, for the reaction-time was actually longer and the probable error was larger after the intermission of two weeks than before it. He had played on the football team all the fall, and that possibly accounted for the quick reaction-time at the first three sittings. His first three tests plainly indicate the effects of previous training, for his responses were much quicker and much more regular than for any of the other subjects tested. He was the only athlete included in Series I, II, and III. The increase in reaction-time and decrease of regularity for *I*, after the intermission of two weeks, may have been due to the fact that during this time he had not taken any special training, having given up the gymnastic exercise just prior to the first series of tests.

Comparison of Series I, II, and III.

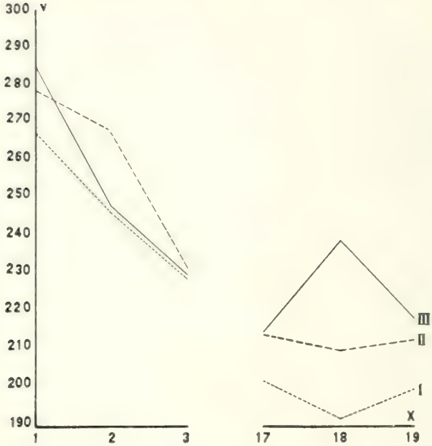
The average results of each group of subjects are given in Table IV.

TABLE IV.

	Average.			*Probable error.*		
	Series I.	Series II.	Series III.	Series I.	Series II.	Series III.
Loud sound, before	233	259	254	34	27	33
Loud sound, after	199	207	222	16	19	20
Decrease	34	52	32	18	8	13
Weak sound, before	240	257	255	35	27	35
Weak sound, after	204	211	227	18	18	20
Decrease	36	46	28	17	9	15

In regard to the average reaction-time there is a decrease for Series I (exercise) and II (thinking) as compared with Series III. In regard to the probable error no such relation is observed. Although the number of subjects was very limited, it seems reasonable to conclude that exercise and "thinking" increase the rapidity of response.

The average results of each group for each sitting are shown for the loud sound in Fig. 1, for the weak sound in Fig. 2, and the average probable errors in Figs. 3 and 4. The numbers on the horizontal axis indicate the days of the experiment; on days 1, 2, 3 the first records were made; then followed the interval, and on the 17th, 18th and 19th days the final records were taken.



300
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280
270
260
250
240
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190
Y
X
1
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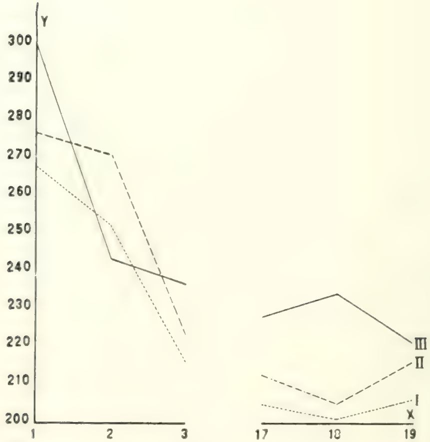
III
II
I

FIG. 1.



Y
300
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220
210
200
X
1
2
3
17
18
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III
II
I

FIG. 2.

The curves show in all cases a steady decrease in the average and the probable error during the first three days. On the 17th day the records are all somewhat smaller than on the 3d day, indicating perhaps some

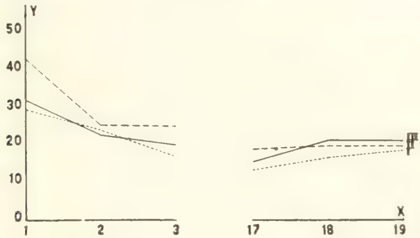

FIG. 3.

decrease due to persistence of impressions from the first experiments. If this same phenomenon should be observed in more extended experiments, it would point to active processes remaining after the reaction had been made.

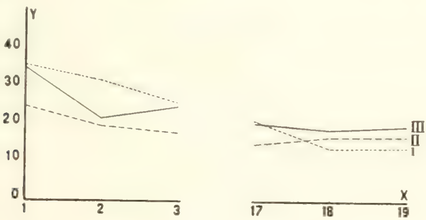

FIG. 4.

Fourth series of experiments.

A comparison was attempted between men who did not take any systematic exercise and those who were athletes at the height of training. The groups were made up as follows:

Group I.

Britan, age 25, weight 160 lbs., slightly nervous temperament, health robust, graduate student in philosophy.

Alexander, age 34, weight 155 lbs., very nervous temperament, health medium, graduate student in the Divinity School.

Cleland, age 28, weight 135 lbs., phlegmatic temperament, health medium, graduate student in geology.

Matsumoto, age 35, weight 145 lbs., phlegmatic temperament, health good, assistant in the Psychological Laboratory.

Geiser, age 30, weight 135 lbs., nervous temperament, health good, assistant in history.

Taylor, age 32, weight 145 lbs., phlegmatic temperament, health good, fellow in philosophy.

Mitchell, age 25, weight 183 lbs., phlegmatic temperament, health good, assistant in history.

White, age 30, weight 155 lbs., extreme nervous type, health good, graduate student in the Divinity School.

McAllister, age 30, weight 148 lbs., nervous temperament, health medium, fellow in psychology.

Sincerbeaux, age 25, weight 160 lbs., phlegmatic temperament, health medium, student in the Academic Department.

Group II.

Johnson, age 22, weight 135 lbs., nervous temperament, health good, captain of track team; specialty, pole-vaulting.

Richards, age 20, weight 138 lbs., very nervous temperament, health good; specialty, sprinting.

Fincke, age 22, weight 153 lbs., medium temperament, health good; specialty, hurdling.

Boardman, age 20, weight 152 lbs., slightly phlegmatic temperament, health good; specialty, sprinting.

Hord, age 21, weight 125 lbs., nervous temperament, health good; specialty, pole-vaulting.

Thomas, age 21, weight 154 lbs., nervous temperament, health good; specialty, hurdling.

Barnard, age 21, weight 155 lbs., nervous temperament, health good; specialty, hurdling.

Adriance, age 22, weight 130 lbs., nervous temperament, health good; specialty, pole-vaulting.

The first experiment made on each subject comprised ten records of a complex reaction. After the usual warning a red or a white cord was exposed in the pendulum chronoscope and the subject pressed the key if it was red, refraining if it was white. The reaction thus included discrimination and choice.

This experiment was followed by a series of twenty records in reacting to a loud sound in a telephone. The sound in the telephone was made very strong, in order to produce something of the shock of a starter's pistol.

Finally a series of ten records was made on the complex reaction-time just as in the first experiment. Before taking the second series, each subject was allowed to inspect his previous record and those of other persons.

The experimenter explained to each of the subjects that this was a test of his ability and that it would be compared with other records.

The reason for dividing the records in choice-time into two series was to overcome fatigue, to show the influence of practice, also to note the effect that a knowledge of one's previous record, as well as the records of others, would have upon the choice-time.

TABLE V.

Group I.

Subject.	*A*	*p. e.*	*a*	*b*	*B*	*p. e.*	*a*	*b*	*C*	*p. e.*
Britan	274	53	3	2	261	37	1	4	225	35
Alexander	242	33	5	1	252	26	2	2	194	19
Cleland	316	30	2	0	271	34	0	0	223	23
Matsumoto	301	45	2	3	277	33	0	1	192	21
Geiser	349	23	2	0	352	23	0	0	244	37
Taylor	328	30	4	0	260	50	1	1	223	27
Mitchell	349	55	3	0	369	35	1	0	252	35
White	297	58	3	0	312	30	3	4	156	21
McAllister	314	24	2	1	304	19	1	0	242	19
Sincerbeaux	303	28	1	2	291	30	0	0	224	19
Average	307	38	2.7	0.9	297	32	0.9	1.2	278	26

Group II.

Subject.	*A*	*p. e.*	*a*	*b*	*B*	*p. e.*	*a*	*b*	*C*	*p. e.*
Johnson	252	44	3	0	197	21	2	1	121	9
Richards	181	30	2	1					127	13
Barnard	209	30	3	1	243	33	4	0	129	17
Adriance	257	28	1	0	209	23	2	2	147	13
Boardman	219	43	3	0					171	14
Fincke	260	20	3	0	230	25	1	0	154	12
Thomas	284	33	1	0					165	17
Hord	171	31	3	1	220	19	2	0	160	18
Average	242	32	2.4	0.4	220	24	2.2	0.6	147	14
Difference, I–II	65	6	0.3	0.5	77	8	1.3	0.6	131	12

Group I, those without physical training.
Group II, athletes: sprinters, hurdlers, and pole-vaulters.
A, complex reaction-time, first experiment, ave. of ten records.
B, complex reaction-time, second experiment, ave. of ten records.
C, simple reaction to sound, ave. of twenty records.
a, number of times of false reaction.
b, number of failures to react.
p. e., probable error.
The unit of measurement is $1\sigma = 0.001^s$.

The differences show most clearly the superiority of the trained athlete over the untrained man in the time of both complex and simple reactions

and the special superiority in the latter. There is hardly any proportionate increase in regularity. There seem to be indications of increased control in the small number of mistakes made by the athletes.

A comparison of the *B* records with the *A* records indicates a decided influence of a knowledge of the previous experiments and of the fact that others had done better.

INVOLUNTARY MOVEMENTS OF THE TONGUE

BY

H. C. COURTEN.

Among the many psychological problems as yet only partially solved is that of automatic muscular movement. This is a phenomenon of many different phases, but the most important one, perhaps, is that of automatic movement of the speech organs. So far but little has been done toward its experimental demonstration. HANSEN and LEHMANN[1] satisfied themselves of its existence, but obtained no measurements in regard to it. CURTIS[2] secured some records showing unconscious movements of the larynx during mental action. These seem to be the only direct investigations of this problem.

The apparatus used in my experiments consisted of a ROUSSELOT[3] exploratory bulb (Fig. 1) fitted with a long tube connected to a MAREY

FIG. 1.

tambour. The bulb was of French make, about 1 cm. in thickness (measured in the direction of the tube), and 9 sq. cm. in cross section. This size was decided on only after repeated trials with bulbs of different sizes. The records were made on a kymograph.

Some difficulty was experienced in finding the correct position for the bulb. At first it was placed far back in the mouth near the palate, because it was thought that the greatest movement of the tongue would be manifested there. But while it was in this position the subject showed a tendency to gag, and could not hold the bulb quietly in place. The most convenient and effective position was finally found to be in the front part

[1] HANSEN UND LEHMANN, *Über unwillkürliches Flüstern*, Philos. Stud., 1895 XI 471.

[2] CURTIS, *Automatic movements of the larynx*, Amer. Jour. Psychol., 1899 XI 237.

[3] ROUSSELOT, Principes de Phonétique Expérimentale, 86, Paris 1897.

of the mouth between the teeth and the tip of the tongue. Thus the tongue was in a fairly natural position, and the slightest movement was recorded. The bulb was entirely unaffected, in this position, either by pulse or by respiration.

The subject was required to do several kinds of mental work without speaking, such as reading prose and poetry, thinking out the etymological relations of various English words ('exhortation,' 'advocation,' etc.), reading Scotch dialect, French, or German, reciting the alphabet, committing verse to memory, etc.

The subject was seated in a chair with his back to the instrument and the bulb carefully adjusted in his mouth. The kymograph was then set in motion and the subject was asked to keep his mind as free from thought as possible. After a short tracing had been secured during mental rest, the subject was told to perform some of the tests mentioned above. In all nine experiments were made. The accompanying figures give typical results.

FIG. 2.

The first part of the tracing in Fig. 2, as far as the dividing mark, was made during a period of mental rest; the second part, during the reading of English prose.

FIG. 3.

Fig. 3 shows a record made after the subject had been requested to remain as free from thought as possible; but after the record had been made he confessed that he had, unintentionally, been thinking of the experiment. The record is significant inasmuch as it shows that movements do occur, in consequence of mental action, without the knowledge of the individual.

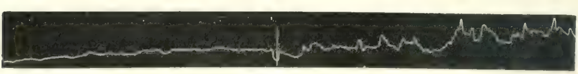

FIG. 4.

Fig. 4 shows the results of a second experiment with the same subject as for Fig. 3. In this case, as the relatively straight portion of the trac-

ing seems to show, the subject was successful in suppressing thought. The test producing the undulations consisted in committing verse to memory.

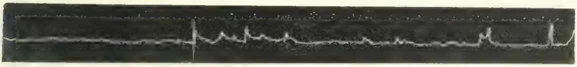

FIG. 5.

The tracing in Fig. 5 shows the physiological effects of reading French, and that in Fig. 6, of reading English. The difference was intensified

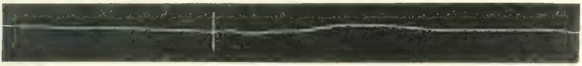

FIG. 6.

by the fact that the English selection was totally devoid of emotional passages.

FIG. 7.

Fig. 7 shows a record made while the subject was mentally reciting the alphabet.

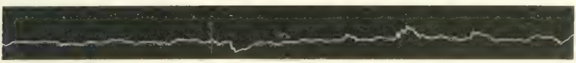

FIG. 8.

Fig. 8 shows a record made while reading German.

The subjects of this set of experiments were a mechanic, two sophomores, three freshmen, a junior and a professor of Yale College. It is not to be understood from the above figures that all tracings showed an equal degree of movement. It can be said, however, that no record was taken which did not show some movement. It was noticed that the tongue was more strongly influenced during intense thought than during less active thinking, and that reading a dialect, or language unfamiliar to the subject, produced a greater effect than the reading of English. But regardless of all variations in the results obtained, the fact was established that unconscious movements of the tongue do take place during mental action.

PHONETIC NOTATION

BY

E. H. Tuttle.

I. General principles.

In order to discuss speech sounds in a concise and intelligible manner, we must represent them by signs of fairly definite meaning. Ordinary orthography will not do for this purpose, since in nearly every language there is more or less of irregularity in the relation of spoken words to their written forms. Thus, in English, groups of letters are often used for simple sounds, and conversely; 'rough' = 'ruff,' 'sword' = 'soared,' 'phlox' = 'flocks.' In German 'wird' = 'wirt(h),' 'stadt' = 'statt,' 'viel' = 'fiel,' 'feld' = 'fällt,' 'so(o)le' = 'sohle,' —z rimes with —ds, —ts, etc. In French 'car' = 'quart,' 'sans' = 'sang' = 'sens' = 'sent' = 'cent,' 'saoul' = 'sous' = 'soue,' 'ceins' = 'ceint' = 'cinq' = 'sain' = 'saint' = 'sein' = 'seing,' 'souhait' = 'soi.' In Italian half the letters of the alphabet are used in two or more different manners.

It is evident that we need for scientific purposes a sign system free from such serious faults as these. It should, however, be kept in mind that to construct and employ a mathematically exact system would be practically impossible, because of the linguistic and physiological differences between individuals.[1] Many of the phonetic symbols used here must therefore be understood as representing *groups* of slightly different articulations, sounds or properties.[2]

One of the first points to be considered is how to distinguish phonetic letters from orthographic ones. Many writers, overlooking the importance of this consideration, make no distinction between orthographic and phonetic spelling, with the result that their work is sometimes unintelligible, or, worse yet, misleading. Thus, when a French writer speaks

[1] The formula system of representing sounds is evidently intended as an approach to mathematical accuracy, but can hardly be called a phonetic transcription in the proper sense of the term; it is rather a description in a sort of physiological shorthand. (Jespersen, Fonetik, Köbenhavn 1897–1899.)

[2] The fact that speech does not consist of a series of adjacent sounds independent of one another, as implied by an alphabetic representation, will be left out of account here.

of "*u* allemand" or "y anglais,"[1] we are more or less in the dark as to what he is talking about.

In some cases it may be found convenient to use phonetic and orthographic signs in combination; thus for instance in a discussion of stress or pitch, much space can be saved by writing "^{1}Ja^{0}pa^{2}nese" instead of "'Japanese' ^{1}*dʒæ*^{0}*pə*^{2}*niis*," where the particular vowels and consonants used are of no especial importance. But this freedom should not be extended too far; such mixtures as "*mamzel, vla, dja, syisi, membrə*,"[2] are highly objectionable, as they might seriously mislead a reader not perfectly familiar with the language represented.

As the scientific notation employed in this article contains roman[3] letters, it will be enclosed in brackets, to prevent confusion with orthography. The English textual system is in italics, which will permit dispensing with the [ ].

Another important point in phonetic work is consistency. This also has received none too much attention, even from the best writers. PASSY uses two different modifiers on the same page[4] to indicate syllabicness. ROUSSELOT uses "*h*" for the two quite distinct fricatives of German 'hage.'[5] ELLIS employs the sign (') in two apparently quite different ways: (') = [*ə*], and ('n) = syllabic or long [*n*];[6] another phonetician coming upon a text containing numerous (')'s, mistranscribes it[7] into an unpronounceable form containing syllabic *v*, *z*, etc. SWEET transcribes the Arabic dental fricatives sometimes by "ð þ" and sometimes by "*ð ϑ*," without apparent reason for this distinction.[8] STORM uses "*æ*, ɛ, *ę*" sometimes for the same vowel (that of 'air'), sometimes for different sounds.[9] HEMPL seems to use "f" and "v" with the same value.[10] SIEVERS employs "r^i" to mean *i*-modified *r*, "e^i" to mean *e* followed by nonsyllabic *i*, and "'" to indicate a half-sonant *ə*.[11] There is a similar inconsistency in the use of superior letters in the system of JESPERSEN,[12] and in that of MURRAY.[13] We find one of the vowels

[1] ROUSSELOT, Principes de Phonétique Expérimentale, 157, 181, Paris 1897.
[2] STORM, Englische Philologie, 2te auflage, 187, Leipzig 1892.
[3] I use "roman" in contrast to italic; "Roman" in contrast to Greek, Russian, etc.
[4] PASSY, Sons du Français, 5[e] édicion, 58, Paris 1899.
[5] ROUSSELOT, Principes de Phonétique Expérimentale, 410, 554, Paris 1901.
[6] ELLIS, Early English Pronunciation, 10, London 1869–1889.
[7] SIEVERS, Grundzüge der Phonetik, 4te auflage, 41, Leipzig 1893.
[8] SWEET, Practical Study of Languages, 31, 233, New York 1900.
[9] STORM, as before, IX, 56.
[10] HEMPL, German Orthography and Phonology, 156, xxx, Boston 1898.
[11] SIEVERS, Grundzüge der Phonetik, 5te auflage, 185, 180, 110, Leipzig 1901.
[12] JESPERSEN, Fonetik, 229, 515, Köbenhavn 1897–1899.
[13] MURRAY, New English Dictionary, vol. 1, p. xxv, Oxford 1888.

(a small "0") that is defined as "short" in HAMILTON's notation used with the length-modifier, and a vowel ("ɑ") that is defined as "long" used for the short vowel of 'not.'[1] In PASSY's system the character "ɹ" is used in three entirely different ways: to indicate a consonant; to indicate qualitative modification of the preceding vowel; and to indicate both qualitative and quantitative modification.[2]

Of equal importance with typographical consistency is linguistic consistency. SWEET transcribes English "long e" as vowel + consonant, and then gives 'sea' as an illustration of a final long vowel.[3] After criticising SWEET for calling English ā and ō vowel diphthongs, but ē and oo vowel-consonant groups, SIEVERS transcribes 'ye' with a diphthong, and 'wound' with a simple long vowel.[4] HEMPL calls w in 'way' a consonant, but y in 'yet' a vowel,[5] although these sounds are really of the same general character. Some of the dictionaries[6] transcribe 'allow' with two *l*'s, although only one has been pronounced during the last ten or twelve centuries. Until very recently the American Dialect Society has used "*o* as in *not*" along with "*ô, ou* as in *no*" and "*oi* as in *coin*,"[7] notwithstanding the confusion that is liable to arise from such a system.[8]

Consistency in the physiological basis of a transcription is also very desirable. When we find a fricative defined as being similar to the Arabic and German glottal catch, and the sounds [*m*] and [*n*] called fricatives, while [*n̦*] and [*ɣ*] are classed as occlusives,[9] we feel tempted to doubt the accuracy of certain other statements made by the same writer. The two Italian z-sounds are often described as simultaneous *t* and *s*, *d* and *z*,[10] although such a combination of stoppage and non-stoppage of the air-current is physically impossible. One well-known sound system divides the vowels according to the assumed tongue-positions into 'front,' 'mixed' and 'back,' and 'high,' 'mid' and 'low';

[1] [HAMILTON,] *With the linguists*, Herald, April 1902 (Toronto), p. 90.

[2] *Exposé des principes de l'association phonétique internationale*, Supplément au Maitre phonétique de novembre 1900 (Passy), p. 8, 12, 15, 18.

[3] SWEET, Primer of Phonetics, 21, 43, Oxford 1890.

[4] SIEVERS, as before, 163.

[5] HEMPL, as before, xxx, xxvii.

[6] [MARCH,] Standard Dictionary of the English Language (Funk), New York 1895. [BARNUM,] Webster's International Dictionary of the English Language (Porter), Springfield 1901.

[7] Dialect Notes, vol. i, p. 27, vol. ii, p. 190.

[8] Dialect Notes, vol. i, p. 233, 452.

[9] ARAUJO, Fonétika Kastellana, 24, 36, 37, Madrid 1894.

[10] D'OVIDIO UND MEYER, *Die Italienische Sprache*, Grundriss der Romanischen Philologie (Gröber), 1ter band, 491, Strassburg 1888.

ELLIS, as before, 800.

the last class is defined as having the lowest possible position of the tongue,[1] the results of such a definition being apparently overlooked. In pronouncing a 'low-front' vowel, where is the back of the tongue? It cannot be above 'high' nor below 'low'; it cannot be 'high' nor 'mid' (for either of these positions would produce 'back' vowel), and consequently must be 'low.' In other words 'low-front' is identical with 'low-back.' Further discussion is hardly needed to demonstrate the frailty of the whole system.

The legibility of phonetic characters is an important consideration. In one system we find "s" and "s" used for different sounds.[2] SWEET uses for [γ] and for [ʒ] two very similar signs[3] — so similar, in fact, as to be misprinted in one of his own books.[4] Frequently "*æ*" and "*æ*" are used in the same alphabet.[5] VIETOR transcribes two German sounds by "*j*" and "*j*."[6] PASSY employs "*g*" and "*ɡ*" for two sounds as nearly distinct as *s* and *t* in his so-called "international alphabet"; [7] this, of course, makes difficult reading in texts where both signs are used.[8] LEPSIUS uses a "*t*" with a microscopic "*s*" beneath it in transcribing Slavic languages, because these happen to represent the sound group *ts* by a single letter.[9] ROUSSELOT often employs minute superscript letters as modifiers; [10] in the case of nasalized "*a*" the tilde is so small that it might easily be mistaken for a macron or grave accent.[11] The more new types an alphabet has, the less easy will it be to read and to remember; this reason, as well as the one mentioned below, explains to a large extent the failure of new-type alphabets to come into common use. The illegibility of his organic alphabet in its script form has been admitted by SWEET himself; [12] for the ordinary educated person the printed characters are extremely hard to read.

[1] SWEET, as before, 14.

[2] Century Cyclopedia of Names (Smith), vii, New York 1899.

[3] SWEET, as before, 97, 98.

[4] SWEET, Practical Study of Languages, 17, New York 1900.

[5] VIETOR, Elemente der Phonetik, 4te auflage, Leipzig 1898.
VIETOR, German Pronunciation, 2d edition, Leipzig 1890.
SIEVERS, Grundzüge der Phonetik, 5te auflage, Leipzig 1901.

[6] VIETOR, as before.

[7] Maître Phonétique (Passy), since 1894.

[8] VIETOR, Aussprache des Schriftdeutschen, 4te auflage, Leipzig 1898.
DJELALI, Contes et Chants Arméniens, Paris 1899.

[9] LEPSIUS, Standard Alphabet, 2d edition, London 1863.

[10] ROUSSELOT, Principes de Phonétique Expérimentale, 332, 614, Paris 1897-1901.

[11] ROUSSELOT, as before, 224.

[12] "As I had some difficulty in deciphering your postcard—which isn't *your* fault but the fault of visible speech which I'm afraid is quite unsuited for practical work—I write in Roman letters." SWEET, quoted by SPIESER, *lautfrift*, Maitre Phonétique, 1895 X 139.

It is not sufficient that a system should not be difficult to read ; it must not be unpleasant to read, if it is to be at all widely adopted.

In order to be pleasant to read, a textual alphabet should be as uniform as possible ; a mixture like "iŋgli∫ t∫aild" is highly objectionable, where the italics serve no phonetic purpose.[1] An exception to this principle may be made, however, where it is desirable to call attention to certain sounds. Thus in an italic text for English students it might be well to transcribe the Russian palatalized consonants or the French orinasal vowels by roman letters, or by heavy-face ones.

Compactness is also an important feature of an alphabet. The clumsiness of using several characters for a single sound becomes a serious waste of space in cases like "œœ"[2] for long [ɜ], and "p[ʰ]"[3] for English *p*. Practical experience has shown that it is best to indicate quantity, stress and pitch by separate modifiers ; these signs should be as small as is compatible with legibility. In a scientific notation the use of modifiers may be considerably extended.

The employment of many diacritics should be avoided ; a transcription with a mark over or under nearly every other letter, or with two and three marks on some, gives to the printed page a repellant aspect.[4] A single word like "*žü·žé*" (Vietor's notation) = French 'jugeait' [*ʒy·ʒɛ*] will illustrate this fairly well. In the system of Lepsius, the second vowel of French 'emprunte' is written "*ǫ*" with a straight line under it, another over it, a tilde over this, and an acute accent over the tilde ; in Rousselot's alphabet the corresponding sign is "*œ*" with a vertical line under it, a grave accent, a macron, and a tilde over it.

A fourth requirement is that, so far as possible, familiar letters should not be used in unfamiliar ways ; a transcription like "psjç"[5] for French 'monsieur' is very disagreeable to a person familiar with Greek. In English, where every letter is used in more than one way, any fixed use must be to some extent unfamiliar. For the vowels *a, æ, e, i, o, u, y*, we can hardly do better than return to the Old English values ; the consonants should, of course, be used with their commoner or older sounds, except in the case of the superfluous ones (*c, j, q, x*).

A phonetic notation, to be suitable for general use, should be easy to print with types now in common use. Although Passy has recently

[1] Sweet, Practical Study of Languages, 17, New York 1900.
[2] Sweet, as before, 41.
[3] Passy, Sons du Français, 5e édicion, 120, Paris 1899.
[4] Vianna, Exposição da Pronuncia Normal Portuguesa, 99, Lisboa 1892.
Vietor, German Pronunciation, 2d edition, 113, Leipzig 1890.
[5] Sweet, History of Language, 28, New York 1900.

denied the validity of this proposition,[1] he practically admits it by using in one of his most important books[2] his easily printed sonant-velar-fricative sign instead of the proper new character to represent the occlusive [*g*].

The casting of new types requires a considerable outlay of time and money, and consequently many writers, publishers and printers would refuse to have anything to do with a phonetic notation containing them. On this account the various new-type systems that have been constructed, including those now employed by BELL[3] and by SWEET,[4] seem to be total failures so far as general adoption is concerned. The numerous Roman-basis alphabets now used all seem to require a larger or smaller number of new types, and are therefore all equally unsuited for practical use; for the employment of a single specially cast character makes a system almost as difficult to use as would the employment of a hundred.

While it is highly desirable that new types should not be used, it is perhaps even more important not to waste old types that are readily available and suitable for phonetic purposes. In the systems of ELLIS and of LEPSIUS capitals are used as in orthography, without any phonetic significance; this greatly increases the number of new types in one, and of digraphs in the other, besides making the alphabets much more difficult to use. In one of the best of the dictionaries[5] we find the same sound represented in different ways: 'come' "cum," 'came' "kêm." Such inconsistencies are not permissible in a phonetic alphabet; a system containing them is really only a reformed spelling, not a phonetic one.

II. DETAILS.

a. Types used.

The most readily available types are of course those of the Roman alphabet: *a b c d e f g h i j k l m n o p q r s t u v w x y z*; the corresponding capitals may also be employed in a scientific notation: *A B C D E F G H I J K L M N O P Q R S T U V W X Y Z.* As small capitals are made only in roman, and as many of them are more or less similar to lower-case letters (C I J K O S U V W X Z), no use of them will be made here.

Of the "accents" commonly made for English fonts, the following are used: *à è* etc., *â ê* etc., *ë ï ö ü ç*; of the ligatures, *æ fi*; of the

[1] PASSY, *l ynite fɔnetist*, Maître Phonétique, 1901 XVI 106.

[2] MICHAELIS ET PASSY, Dictionnaire Phonétique de la Langue Française, Hannover 1897.

[3] BELL, Sounds and their Relations, Salem 1881.

[4] SWEET, Primer of Phonetics, Oxford 1890.

[5] [MARCH,] Standard, as before.

full-sized numerals, *2 3*; of the punctuation marks, , : . ' - ! † ‡ ||. The Danish alphabet furnishes two very serviceable types, *æ* and *ø*, and Greek a large number: β γ δ ε ζ η θ ι λ ξ ρ σ τ φ χ ψ ω ʽ ´ ` ˆ ˇ. These foreign types are in a few cases chosen arbitrarily; in most instances they are used with approximately their native values, or else are to be considered as modifications of similar Roman letters.

The number of available characters can be increased by means of different styles of type — as roman, heavy-face, superior, inferior — and by means of cut and turned letters. It should be noted that some types are not suitable for use in this last manner; turned b, d, n, p, q, s, u, x, z are rather hard to read. Turned o is indistinguishable from direct o, unless the alinement is very uneven: the use of the two together[1] makes a text practically illegible.

Cut types are of two sorts, face-cut and body-cut. The process of body-cutting is sometimes undertaken by printers in preference to ordering special characters from a foundry, such as ś, accented æ, capital ç, etc. It is very objectionable not only by reason of the considerable time and expense involved, but because the cutting and adjusting are often poorly done, lying as they do outside of the typesetter's regular line of work. Face-cutting is a comparatively simple matter; it should not, however, be employed for any great number of signs. In the *textual notation* (English) of this article, only one face-cut type is needed, the length-sign ', made from an exclamation point. Certain kinds of face-cutting should not be undertaken without proper consideration of the other principles involved. Thus the use of dotless i or j may seem a very simple expedient; but to use "i" and "ı"[2] or "*j*" and "*ȷ*" in the same alphabet is a serious mistake, because in printing the dots are sometimes broken off, so that whenever the reader sees one of these types he is uncertain whether the mutilation is accidental or intentional.

b. Scientific notation.

A scientific notation should be so constructed as to be capable of providing a suitable transcription for any speech sound. An attempt will be made here to outline such a sign-system, due regard being paid to the principles considered in the preceding pages, and to the present usages of philologists.

One of the first things to be symbolized is the motion of the air-current. While the exact method of indicating this is not very important, it is

[1] ARAUJO, as before, 143.

[2] *Exposé* etc., as before, p. 14.

important to have *some* method, in order to avoid the confusion that is to be found in some writers.[1]

[||] respirate (current due to lung-action).

[‡] irrespirate (current not due to lung-action).

[†] ingressive (current passing into vocal organs from external air).

[⫰] egressive (current passing from vocal organs to external air).

Irrespirate and inspirate sounds are often used in interjections, as [‡†ʈ], [||†*ve⫰s*]; otherwise ordinary speech is generally expirate in European languages. The modifiers [||] and [⫰] will therefore be usually understood, where the contrary is not indicated.

i. Consonants.[2]

LABIAL

BILABIAL

occlusive, [*b*; *p*] F. b; p.

corresponding orinasal, [*m*] E. m.

vibrant, [ϖ] in [*b*ϖ'] expressing a feeling of coldness.

fricative, [β; φ] Dutch w; Japanese f. The sign similar to *υ* used for [β] by PASSY is quite objectionable, because it is a new type; also because in italics it must be printed as "*v*,"[3] the distinction between [*v*] and [β] thus being lost, or else the relation of "*υ*" and "v" must be just reversed, in "*V*" and "*v*,"[4] in order not to disturb the universal association of *v* with v.

strictive, [ω] a rare substitute for E. *r*.

BILABIAL-MARGINAL

occlusive orinasal, [ϖ] [5] a common respiration-articulation with sonancy added; [ϖ'] = "yes."

[1] PASSY, Sons du Français, 5e édition, 110, Paris 1899.

ROUSSELOT, as before, 488.

[2] The terms "consonant" and "vowel" will be used here in the ordinary manner, without discussion of the hitherto undetermined distinction between the two classes. It is noteworthy that the consonants can in nearly all cases be referred to fairly definite genetic conditions, or articulations; while this is not easily done for the vowels, as is shown by the widely differing vowel-theories upheld by various writers.

Where two consonant-signs in the list are separated by a semicolon, the first is sonant, the second surd; where only one is given, it is sonant unless the contrary is stated. Articulations are oral unless stated to be orinasal.

The following language names are abbreviated to their initial letters: American English, British English, English, French, German, Hungarian, Italian.

[3] Maître Phonétique (Passy), 1897 XII 80, 81.

[4] VIETOR, Aussprache des Schriftdeutschen, 4te auflage, 5, 17, Leipzig 1898.

[5] To be associated with *m*.

BILABIAL-DORSAL

strictive, [*w*] E. consonant w; approximately a combination of the labial [*ω*]-position and the lingual [*u*]-position.

LABIOBIDENTAL

surd fricative, [*ɸ*] in [‡†*pɸ*'] sometimes used to urge on horses.

LABIODENTAL

occlusive, [b; p].
corr. orinasal, [m].
fricative, [*v*; *f*] F., I. v; f.

DENTAL

fricative [*ϱ*; *ɥ*]; inspirate [*ɥ*'] is sometimes used as an interjection of pain.

LINGUAL[1]

m a r g i n a l

INTERDENTAL

fricative, [*ð*; *ϑ*] E. sonant th; surd th. The signs "ð þ" for these sounds are in general use among Germanic philologists; the chief objection to them is that italic þ is very similar to certain styles of *p*, so that one is liable to be misprinted for the other.[2] Another important consideration is the fact that many printers, and even type-founders, have difficulty in furnishing italic ð þ.[3] If roman *ð ϑ* are needed, 9 might be used for the latter, while the former could be made by cutting 8; or ð might be considered the roman letter corresponding to italic *ð*. The German printers' crossed d, evidently intended as an improvement on ð, is unsuitable for practical use, being much inferior to it in legibility.[4]

[1] Names of lateral articulations are in italics.

[2] Modern Language Notes, 1901 XVI 154 (c. 307).
EMERSON, History of the English Language, 235, New York 1897.

[3] SWEET, Practical Study of Languages, 135, 233, New York 1900.
SWEET, History of Language, 28, 125, 131, New York 1900.
SIEVERS, as before, 147.
EMERSON, as before, 241.
HEMPL, as before, xxvi, xxviii, xxix, 128.
American Journal of Philology (Gildersleeve), 1901 XXII 427, 430.

[4] Compare LLOYD's Northern English (Leipzig 1899) with SWEET's Elementarbuch des Gesprochenen Englisch (Oxford 1895).

POSTDENTAL

fricative, [*ȡ* ; *σ*] sometimes distinguished in English, though perhaps never distinctive : [*sikσ*'] = [*siksϑs*] 'sixths' ; [*hæzȡəm*] = [*hæzðəm*] 'has them.'

ALVEOLAR

occlusive, [*d* ; *t*] E. d ; t.
corr. orinasal, [*n*] E. n.
occlusive, [*d* ; *τ*]. Independent lateral occlusives are rare ; [‡†τ'] or [‡†τ̬] is used to urge on horses. In words like 'wildly' and 'faultless' the occlusives are necessarily lateral, and do not need to be marked as such in an ordinary transcription.
vibrant, [*r*] Scotch r.
fricative, [*z* ; *s*] E., F. z ; E., F., G. "hard" s.
fricative, [ʒ̬ ; ʒ̥]. Welsh ll = [ʒ̥] ?
strictive, [*ɹ*] Londonese consonant r ?
strictive, [*l*] G. l.

ALVEOLAR-DORSAL

strictive, [*ɫ*] E. l ; a [*l*] with approximately [*u*]-modification of the dorsal tongue position.

CEREBRAL

[d t n r z s ɹ l] differ from [*d t n r z s ɹ l*] in having the tongue point much farther back.

PAGINAL

fricative, [*ʒ* ; *ʃ*] F. j, E. si in 'vision' ; E. sh, F. ch, G. sch. These signs, now coming into common use, are preferable to the German philologists' ž š by reason of their greater legibility and ease of writing, and also because the Bohemian accented letters are difficult to obtain in this country. The chief objection to *ʃ* is its similarity to ordinary *f*; this may be avoided by using an italic f with no kern at the bottom. If a roman character is needed, the letter ſ might be used ; the resemblance could be increased by removing one serif. The articulation of the paginals seems to be quite variable ; the assumption that [*ʃ*] is necessarily formed farther back than [*s*] [1] is as incorrect as the older theory that [*ʃ*] is simply [s].[2]

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[1] SIEVERS, as before, 131.
[2] HENRY, Comparative Grammar of English and German, 27, London 1894.

d o r s a l

PREPALATAL

occlusive, [*ɟ* ; *c*][1] H. gy ; ty.

corr. orinasal, [*ɲ*] Italian gn, Spanish ñ.

fricative, [*j* ; *ç*] G. j g in 'jäger'; ch in 'mächte, bücher.'

fricative, [*ʑ* ; *ɕ*].

strictive, [*j*] E. y in 'year,' e in 'ewe, few.' This sound is about as distinct from [*j*] as [*z*] is from [*ð*] ; it is a serious mistake to use a single sign, as some writers do, for both sounds. The relation of [*j*] to the strictive [*ɥ*] of F. 'lui' seems to be the same as that of the vowels [*i*] and [*y*].

strictive, [*ʎ*] I. gl(i). Passy's double use of italic *ʎ*,[2] sometimes for this sound, and sometimes for a *h*-sound, is objectionable ; *ʎ* might be used instead of *ʎ*, but the other form (*ɦ*) should not.

PALATOVELAR

occlusive, [*g* ; *k*] E. "hard" g ; k.

corr. orinasal, [*ŋ*] E. ng in 'young,' n in 'younger, uncle.'

fricative, [*γ* ; *χ*] G. g in 'lage' ; ch in 'macht, buch.' Passy's use of "g" for the sonant fricative[3] is extremely objectionable, because it involves, in a roman notation, the use of a new type for the sound [*g*], and because the letter g is in most European languages commonly associated with the occlusive. The unpractical character of Passy's notation is shown by the fact that several books[4] represented as using it employ "g" as an occlusive, while one[5] uses "g" for [*g*] about twice as often as for [*γ*].

POSTVELAR

occlusive, [g ; k].

corr. orinasal, [ŋ].

vibrant, [*ʀ*] a common form of German r (uvular trill).

fricative, [q ; x]. A roman form more similar to *γ* could be made by cutting off half the loop of q.

strictive, [*ʁ*].

strictive, [*ʟ*] Russian "hard" l? The Polish crossed *l* (*ł*) often used for this sound is objectionable because it is not easy to obtain, even from type-foundries, and because it is easily misprinted as *t*.[6]

[1] To be associated with *j ç*.

[2] Maître Phonétique, 1897 XII 79, 1900 XV 66.

[3] Passy, Sons du Français, 5e édicion, 111, Paris 1899.

[4] Michaelis et Passy, as before.

Passy, Le Français Parlé, Leipzig 1897.

Lloyd, Northern English, Leipzig 1899.

Spiers, Senior French Reciter, London 1902.

[5] Rippmann, Elements of Phonetics, 7, 14, 35, 40, 52, 57, 79, etc., London 1899.

[6] Maître Phonétique (Passy), 1893 VIII 109, 1902 XVII 1, 108.

LARYNGEAL

GLOTTAL

surd occlusive, [*ʔ*] G. "glottal catch."

surd fricative, [*h*] E. h, G. h.

Other sounds of this class seem to be distinguished in oriental languages; but until their articulations are better known, it is hardly necessary to provide signs for them.

ii. Vowels.[1]

NORMAL

[i] F. i, G. ie, I. i.

[*i*] G. open i.

[e] F. é, G. eh, I. close e.

[*e*] G. e in 'feld,' ä in 'fällt.'

[ɛ] F. è, ê, G. äh, I. open e.

[*æ*] E. a in 'pats,' A. a in 'past.'

[a] F. a in 'part, page, patte.'

[*a*] I. a.

[ɑ] F. â.

[*å*] E. au, aw.

[*ɔ*][2] I. open o.

[*o*] G. o in 'sollte, sonne.'

[o] F. eau, G. oo, oh, I. close o.

[*u*] G. open u.

[u] F. ou, G. uh, I. u.

[1] As the relations of the vowels are a matter of much dispute, it may be well to give some reasons for the classification adopted here.

(1) The normal series forms, when whispered, a fairly continuous pitch-scale; (2) in pronouncing the normal vowels simultaneously with [*z*], the easiest is [*å*]; passing from this in either direction, they become more and more difficult, [u] and [i] being about equally hard; (3) in pronouncing the normal vowels, in the order given, the tongue-point is gradually retracted; (4) the vowels [*a*,], [*å*,], [*o*,], [*u*,] can be distinguished fairly well, while attempted nasal [*æ*], [*e*], [*i*] give [*ɔ*,], their dorsal articulations being in front of that of [k]. This seems to show that the normal vowels lie in a single continuous series, and that the *o*- and *u*-articulations are behind the uvula.

(5) The *y*-sounds can be formed without "rounding," and the *i*-sounds with it, without any considerable change in quality; (6) both these classes of sounds can be pronounced simultaneously with *m*; (7) attempted simultaneous [*j*] and [*u*] gives a sound very similar to [*y*]. This apparently proves that the *y*-sounds are combinations of *u*- and *i*-positions (slightly reduced?), and are not (necessarily) "rounded" *i*-sounds.

[2] To be associated with *o*, as *ε* is with *e*.

REDUCED

[ì] intermediate to [i] and [ə].
[*ì*] intermediate to [*i*] and [ə].
[è] intermediate to [e] and [ə], etc.[1]

NEUTRAL

[ə] simple voice, the unaccented vowel of E. 'fungus, wondrous.'

NORMAL COMPOUND

[y] combination of [u] and [i], F. u, G. üh, Danish close y.
[*y*] com. of [*i*] and [*u*], G. open ü.
[ø] com. of [o] and [e], G. öh, F. close eu.
[*ø*] com. of [*o*] and [*e*], G. open ö.
[ɜ] com. of [ɔ] and [ɛ], F. open eu.
[ʌ][2] com. of [*ȧ*] and [æ], B. stressed u in 'fungus'?

REDUCED COMPOUND

[ï] com. of [i] and [ù], Russian ы.
[*ï*] com. of [*i*] and [*ù*].
[ë] com. of [e] and [ò], etc.[3]

The use of most of the vowel signs requires little comment. The sign *œ*, often used for [ɜ], is avoided here in order to permit the use of *æ* instead of *æ*, and because of its conflicting associations; if *œ* is to be used at all, it should be for the sound intermediate to [*e*] and [*o*].[4] If roman ɜ is needed, Russian з-like *z* might be used. The sign *å* seems necessary for several reasons. The sounds *æ* and ɜ are distinguished by SWEET[5] and by PASSY[6] as here, but they use the single letter ɔ for the vowels of B. 'knot, nought,' as well as for the somewhat different one of F. 'port, note.' The distinction is just as important in the latter case as in the former, and is therefore indicated here by *å*—ɔ. The character *å* indicates the sound intermediate to the *a*-group and the *o*-group, just as æ does the one between the *a*-group and the *e*-group; and it is especially suited to American English, because commonly written with an a: 'all, awl, haul, talk, watch,' etc.

[1] Indicated by a grave accent over the corresponding normal vowel.
[2] To be associated with *a*.
[3] Indicated by two dots over the corresponding normal vowel.
[4] ATKINSON, *ælfəbit*, Maître Phonétique, 1900 XV 61.
[5] SWEET, Practical Study of Languages, New York 1900.
[6] PASSY, Sons du Français, 5[e] édicion, Paris 1899.

iii. Modifications.

LENGTH

In many languages the time-element (duration, quantity, length) of sounds is an important factor of speech. In the Germanic tongues similar long and short vowels are often appreciably different in quality: compare English 'fairy—harry, wren—rein, pick—pique, good—food,' etc.; from which fact there has arisen the common notion that a difference in quality necessarily accompanies difference of quantity.[1] This idea however is readily proved false: in music the "short" vowel of 'sat' may in one measure be sung to an eighth note, and in another to a half note, with any noticeable difference of quality. In some languages, as French and American English, words are distinguished solely by the duration of their vowels; quantity signs are therefore often indispensable in a phonetic notation.[2]

The indication of length by doubling a letter is objectionable not only by reason of its sprawliness,[3] but because it renders impossible, in a textual notation, a distinction like that of the diphthong *ii* in 'key-ring' and the simple long *i* in 'hearing.'

The use of a colon (:) to indicate length has been adopted by many writers.[4] This may do well enough in the Romance languages, where final vowels are commonly short, but it is unsuitable in the Germanic ones, because at the end of a word it may easily be mistaken for and misprinted as a punctuation mark.[5] A modified form with angular dots[6] is rather illegible, resembling i with the dot broken off, so that in transcription 'call' looks like 'coil,' 'pause, paws' like 'poise,' etc.[7]

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[1] SOAMES, Introduction to Phonetics, 2d edition, 54, London 1899.

[2] Much of the otherwise excellent work published by the American Dialect Society is made more or less unintelligible by the lack of any systematic method of marking vowel-length.

[3] SWEET, Primer of Phonetics, 100, Oxford 1890.

[4] MICHAELIS ET PASSY, as before.

PASSY, Le Français Parlé, 4e édition, Leipzig 1897.

VIETOR, Aussprache des Schriftdeutschen, 4te auflage, Leipzig 1898.

RIPPMANN, Elements of Phonetics, London 1899.

SPIERS, as before.

[5] Yet even in Romance speech interjections sometimes have final long vowels, so that confusion may occur. For actual examples, see PASSY's Français Parlé (4th edition): "səkrɛ:", page 9, line 3, has a short vowel, while "o :", p. 11, l. 8, is long.

[6] PASSY, Eléments d'Anglais Parlé, 3e édition, Paris 1900.

[7] SPIERS, *A moyen*, Maître Phonétique, 1902 XVII 87.

The popular method of marking quantity by the macron and breve, ā, ă, ē, ĕ, etc., is unsuitable for an extensive scientific notation, because it requires the casting of new types for long and short æ, å, ɔ, ȯ, ü, n, s, etc. To avoid typographical difficulty, it has been proposed that these marks should be put after the letter modified instead of over it.[1] This plan is unsuitable for a textual notation, because if the marks used are large enough to be properly legible, they take up a great deal of space (half an em or more). As the length sign seems to be the only one needed in ordinary transcriptions, a mark like the macron, but in a different position [ˈ], is proposed here. If other signs are required in more accurate work, the following might be employed:

[ꞌ] over-long.
[˖] half-long.
[˘] short.
[ˆ] extra-short.

These are placed after the sound-sign modified.

PITCH

A common manner of indicating pitch, or intonation, is the use of slanting lines, angles and the like to mean rising, falling, high, etc.[2] This method is (or might be made) a good one, as far as it goes; but if it is used, care should be taken to provide for all necessary variations. In one system,[3] where only level, rising and falling tones are indicated, we find that the "tone-marks are put before the word they modify; if they modify a whole sentence, they are put at the end of it"; using this it would be impossible to distinguish normal "Is he ready?" with a rise throughout from "Is he READY?" with a rise through three syllables and a low tone for the fourth.

To indicate pitch with considerable accuracy, ordinary musical notation is sometimes employed. This system is hardly suitable for textual use, because of its bulkiness; it might be replaced by numerals corresponding to the semitones: [^{0}] for the lowest note of speech (which is usually a little higher than the lowest singable note), [^{1}] for the next semitone, [^{2}] for the next, etc. This provides for a variation extending

[1] VIANNA, *kɔrɛspõdã:s*, Maître Phonétique, 1890 V 105.
HALDEMAN, Analytic Orthography, 83, Philadelphia 1860.

[2] SWEET, Primer of Phonetics, 65, Oxford 1890.
PASSY, Sons du Français, 5[e] édicion, 71, Paris 1899.
SIEVERS, Grundzüge der Phonetik, 5te auflage, 225, Leipzig 1901.

[3] SWEET, Primer of Spoken English, 3d edition, 3, Oxford 1900.

through nine semitones, a range that is not commonly exceeded in speech. If the complete octave were needed, superior 10 could be indicated by [^{X}], and 11 by [^{Y}], the groups [^{10}], [^{01}], [^{12}], etc. thus being left for glides within a vowel or syllable; the second octave could be marked with [*], or some other arbitrary sign. Taking the lowest note as do, we should have these correspondences:

[^{0}] do	[^{3}] ri, me	[^{6}] fi, se	[^{9}] la	[^{*0}] do
[^{1}] di, ra	[^{4}] mi	[^{7}] sol	[^{X}] li, se	[^{*1}] di, ra, etc.
[^{2}] re	[^{5}] fa	[^{8}] si, le	[^{Y}] si	

The pitch-marks should be placed before the syllables modified.

STRESS

The common method of indicating stress in English, by a mark (′) after the accented vowel or syllable, is unsatisfactory because it affords no means of distinguishing ordinary 'wi·thout' from artificial or emphatic 'with·out'; the use of a mark on the vowel is objectionable for the same reason, and because of typographical difficulties with *v*, *ə*, *a*, *ā*, etc. The use of an accent before the stressed syllable, as in one of PASSY's systems, is entirely unsuitable for English texts because of conflicting associations with the ordinary use of the accent. The best system seems to be that employed by SWEET[1]: [·] for strong stress, [:] for medium, and [-] for weak. If other distinctions are required, they might be indicated by combinations of these marks, as [··] for very strong, corresponding to orthographic italics. The stress signs are placed at the beginning of the syllable.

SYLLABLES

The common philological notation for the nonsyllabic function is the mark ̯ under the sound-sign modified. As this requires new types in most cases, a more practical means of indicating a nonsyllabic vowel would be to put the same sign after the vowel: [*a*̯], [*e*̯], etc.

On the analogy of [˘̯] we might use [ˌ], the turned length sign, to indicate a syllabic consonant, without thereby implying any direct or fixed relation between quantity and syllabic function; to use the same sign for these two things, as some writers do,[2] is not advisable, because syllabic and nonsyllabic sounds alike can be either short or long.

[1] SWEET, Elementarbuch des Gesprochenen Englisch, 3te auflage, Oxford 1895.

[2] BELL, Sounds and their Relations, Salem 1881.

LLOYD, Northern English, Leipzig 1899.

In the description of a textual notation given below these signs are not used, since in English the vowel-diphthongs have the second portion nonsyllabic, while syllabic consonants occur only before a consonant or a pause.

SONATION

[*P Φ F θ T*], etc. For capital *ʃ* the letter *ʃ* can be used.

SURDATION

[' ʷ '], etc. If it is necessary to distinguish whispered sounds from surds, they can be indicated by smaller letters printed on the line instead of superior.

EXPLOSION

An occlusive before a pause or another occlusive has little audibility unless it is exploded; in a scientific notation there is need of signs to indicate the explosion: [ˈ] surd, [ʻ] whispered, [’] sonant. The last of these differs from the ordinary vowel ə in having much less duration, and loudness, or sonancy, as may be seen by comparing 'Rhoda' *roudə* with 'road' *roud'*. The sign [ˈ] might be used in a very accurate textual notation to indicate aspiration of surds before any sound, as 'stay' *stˈei*, 'try' *tˈrai*, etc., although in such cases the [ˈ] can hardly be considered a separate sound, but is rather a sign that the following vowel or strictive commences surd.

Where it is necessary to indicate the absence of explosion, this may be done by the use of a period; English 'actor, Bagdad' have [*k.*], [*g.*], not [*kˈ*], [*g’*] like the similar French words.[1]

NASALIZATION

The Polish and Portuguese signs for orinasal vowels have come into general use among phonetists; unfortunately neither system is very practical, since only two letters are available in each (ą, ę; ã, õ). In a scientific notation we can, as HALDEMAN suggests,[2] imitate the Polish sign by a turned apostrophe [ʻ] after the letter modified: Fr. 'vin' [*væʻ*], 'vent' [*vɑʻ*], 'vont' [*voʻ*], 'un' [ɘʻ]. These vowels occur independently in English in some interjections, as [*æʻhæʻ*], [*həʻ*], etc.; they may often be distinguished, but are not distinctive, in cases like [*seʻnd*] 'send,' [*wiʻnd*] 'wind.'

Complete nasality, formed by closing the oral passage at the [k]-position, may be indicated by [,]. While the nasal vowels perhaps do not

[1] PASSY, as before, 121.

[2] HALDEMAN, as before, 46.

occur in any language, they are important for experimental purposes (p. 107, note 1).

Modification of one sound toward another can be represented by the use of inferior letters, as [$ə_ɹ$] in a common American pronunciation of 'winters,' [s_i], [m_i] in Russian '7,' etc.

Advancement of articulation may be indicated by [$_,$], as [$t_,$] postdental, [$t_{,,}$] interdental, [$t_{,,,}$] lingualabial; retraction by [$_\backslash$], as [$t_\backslash$] postalveolar, [$t_\backslash$] postpalatal.

Any modification desired might be indicated by inferior numerals; for instance [r_1], [r_2], [r_3], etc. to show the number of flaps.

c. Textual notation.

A notation used to transcribe a continuous text in a single language must differ somewhat from the scientific notation given above; especially in that digraphs like $a_ɹ$, $a_ɩ$, a_1, a_2, a_3 for simple sounds are not permissible, except for length, stress and pitch modifiers; typographical distinctions like *b*, b, ^{b} should in general be avoided, except where it is desirable to call the student's attention to some unfamiliar modification or class of sounds in a foreign language.

The following list includes signs for the American English sounds that need to be distinguished in an ordinary literary text: sounds that occur only or chiefly in exclamations are not taken into account.

b 'oBey'; *p*[1] 'aPPear'; *m* 'oMit'; syllabic *m* 'cup AND saucer'; *v* 'leaVes'; *f* 'leaF'; *w* 'wight,' 'white' *hwait*.[2]

d 'riDer'; *t*[3] 'wriTer'; *n* 'hoNor'; *n*' 'teNDs';[4] syllabic *n* 'listEN'; *ð* 'eiTHer'; *ϑ* 'eTHer'; *z* 'raises razes'; *s* 'miss'; *s*' 'misTs';[5] *ʒ* 'visIon,' 'age' *eidʒ*;[6] *ʃ* 'fiSH,' 'rich' *ritʃ*,[6] 'nature' *neitʃə*; *r* = [ɹ]

[1] Sometimes pronounced where not written, as 'warm( )th, dream( )t, some( )thing.'

[2] A few speakers do not pronounce the *h* in this and similar words, so that 'white' = 'wight,' 'where' = 'wear,' 'which' = 'witch,' etc.

[3] Often inserted where not written, so that 'Welsh' = 'Welch,' 'dense' = 'dents.' The consonants *d t n l* may become nearly interdental when adjacent to *ð* or *ϑ*, as in 'earthen,' and are slightly retracted after apical vowels, as in 'bartered.'

[4] In careful or emphatic speech the d is often pronounced, so that the word is perfectly distinct from 'tens.' In some pronunciations this long *n* does not seem to be distinguished; likewise with long *l*.

[5] In artificial and emphatic speech the t is often pronounced: *mists*.

[6] In these consonant groups the occlusive and fricative belong the same class; either may be slightly different from ordinary *d t ʒ ʃ*. In my pronunciation the paginals and *r* have about the same lip-position as *w*.

'meRRy';[1] *l* = [*l*] 'hoLy whoLLy'; *ł* 'hoLDs';[2] syllabic *l* 'handLEs'; *g* 'fiNGer'; *k'* 'CHoir Quire'; *ŋ* 'siNGer'; syllabic *ŋ* 'lookINGglass'; *y* 'You Ewe,' 'Hugh hue hew' *hyuu*; *h* 'Hold.'

The surd occlusives, *p*, *t*, *k*, when not followed by an occlusive are usually aspirated, though very weakly before an unstressed vowel. The importance of this aspiration is shown by the fact that English-speakers are liable to mishear unaspirated *p t k* as *b d g*.[4] The consonants *k g ŋ* vary somewhat according to the neighboring sound; they do not however reach the full prepalatal position, as sometimes happens with French gu, qu: [*cεl*] 'quel,' [*ci*] 'qui,' etc. The sonant occlusives and fricatives are usually whispered before a surd; the sonant occlusives become surd after a surd, but are kept distinct from *p t k* by the absence of aspiration; *h* tends towards sonancy after a sonant. The glottal occlusive ; seems to be often substituted for *p*, *t* or *k* between orinasals, as in 'bluntness.'

ii 'SEE SEA, KEY-ring'; [*i*i]; with some speakers nearly [iˈ].

*i*ˈ 'spEAring, pEriod'; [*i*ˈ]; some replace this by a diphthong, *iə*; similarly with long *e*, *o*, *u*.

iə 'spEARS, PIERS PEERS'; [*iə*].

i 'spIrit, bUsy, gIve'; [*i*], nearer to [*ï*] than to pure [*i*].

i 'studdEd studIEd, Accept Except, palAce, lettUce'; unstressed and therefore rather variable, [*i*], [*è*]; only preconsonantal.

ı 'busIest, sixtIeth, cerEal serIal'; unstressed; approximately [e], varying towards [ë] or [*i*]; chiefly prevocalic.

ei 'SAY, WEIght WAIT'; approximately [*è*i], but the second portion seems to vary towards [e]; before *r* or *ə* sometimes replaced by long [*e*].

e 'SAYs, mEn, mEAnt'; [*e*]; before *g* and *ŋ* this may become slightly diphthongal, *ee*, the second part being [e]: 'bEg, lEngth.'

[1] Public speakers and singers often use a *r* = [*r*], with one or more flaps.

Some Americans, like many Londoners, insert *r* as a hiatus-filler after *ə* and long *å*, *a*: 'idea ( ) of, draw( )ing, Shah ( ) of,' etc. This *r* is probably not due, as has been supposed (LLOYD, Phonetische Studien, 1892 V 89), to apicality of the vowels, since it is used chiefly by persons who cannot (or at least do not) employ apical vowels in 'hard, furthers'; it is merely the result of analogy, like so many other speech changes (for example 'nothing—nothin' from a similar variation in present participles; 'we was—he was' from the lack of inflection in all other preterits; 'different than' from 'other than'). Most words ending in *ə*, and many in long *å*, *a*, are written with an r that is regularly pronounced before a vowel; this prevailing duality of pronunciation has simply been extended to all words with these endings.

[2] In artificial or emphatic pronunciation sometimes *houldz*, perfectly distinct from 'holes.'

[3] Sometimes inserted where not written, as 'leng( )th, streng( )th.'

[4] CUST, *Report on Korean*, Transactions of the Philological Society, 1877-8-9, p. 615.

*e*ˈ 'cAring, fAIry, bEArer'; [ɛˈ].

eə̯ 'cAREs, fAIRs, bEARs'; [ɛə˔]; sometimes replaced by *ê*ˈ [ɛˈ˔].

*æ*ˈ 'bAd, pAth, pAst, hALve, smAsh, sAnd, mAn'; nearly the same in quality as short *æ*, with perhaps a slight tendency toward [ɛˈ].[1]

æ 'bAde, hAth, hAst, hAve, sAsh, plAnned, bAn'; sometimes diphthongized to *æe* before *g* and *ŋ*, as in 'rag, rang.'

*a*ˈ theoretical vowel of 'half, path, past,' etc., [aˈ]; used chiefly by those who naturally employ long *æ* or *a*.

*a*ˈ 'ALms, cALmer, fAther'; usually on the [ɑ]-side of [*a*]; in eastern New England perhaps more commonly on the [a]-side.

a 'yAcht, knot, comma, bother'; identical in quality with the long vowel in most parts of the United States.

*â*ˈ 'ARms, hEARt, fARther'; [*a*ˈ˔] or [aˈ˔]; the corresponding short vowel may be distinguished in weak syllables, as 'pARtake.'

ai 'AIsle Isle, AYe EYe, hIgh, hEIght'; [*à*i], the final element as in *ei*; in the South sometimes nearly *a*ˈ (*aə*? *aæ*?).

au 'how, OUt, loUd'; [*à*ù], with second element varying toward [o]; in the South the first element is reduced *æ* or even *e*.

*å*ˈ 'hAll, dAUghter, sought, tAUght, wALk.'

å 'hAlt, wAter, thOUght, cAUght, wAsh, wAtch, squAnder,' identical in quality with the long sound; not distinguished by some speakers, who use *a* or *å*ˈ instead.

oi 'bOY, hOIst'; first portion [*å*], [ɔ] or [*o*], second as in *ei*.

*o*ˈ 'sOAring, pOUring, stOry'; [ɔˈ] or [*o*ˈ]; by some speakers replaced by *å*ˈ.

*o*ə̯ 'wORn wARn, sOARs, pOURs, fORward, fORm, fORce'; [ɔə˔] or [*o*ə˔]; by some not distinguished from long *å*.

ou 'sEW sO, sOUl, cOAt'; [*ò*ù], second portion as in *au*.

ò used by a few speakers in 'whole, wholly, stone, coat, only,' etc.; [*ò*]; obsolescent.

u the unstressed sound corresponding to stressed *uu*, as in 'annual, gradual'; one form of unstressed "long o," as in 'following'; chiefly prevocalic; [*u*] or [o].

u 'good, foot, book, bUsh, cUshion, fUll'; slightly reduced [*u*].

ù one form of unstressed "long o" before a consonant or pause, as in 'follows, disobey'; sometimes distinguishable, though never distinctive, in trisyllabic 'usually, gradually, actually'; variable between [*ù*] and [*ò*].

[1] In eastern New England this vowel seems to be rather uncommon, short *æ* or long *a* being used instead, as in southern England.

*u*ˈ ‘mooring, endURing’;[1] [*u*ˈ].

uə ‘mooRS, endURES’;[1] [*uə*].

uu ‘mood, boot, knEW’;[1] [*u*u], in some pronunciations nearly [uˈ].

ə ‘AUGUSTA, fUngUs, son sUn’; in most parts of the United States the stressed vowel is hardly to be distinguished from the unstressed one, [ə]; often used by restressing for “short o” in ‘what, was, because, from,’ etc.; often inserted where not written, as ‘chas()m, rhyth()m, fi()le,’ etc.

əi a rare form of unstressed “long i”; [əi].

əu one form of unstressed “long o,” as in ‘follows, following.’

‘altARS altERS, mannERS manORS’; [əˌ], might be transcribed as syllabic *r*, from which it is very slightly different.

*v*ˈ ‘fURze fIRS, sERf sURf’; a point-modified vowel with dorsal articulation rather variable, from [ɜ] nearly to [*ë*], [*ö*], [*a*], [*ə*]; in New York and vicinity often *vi*, with same final element as in *ei*, *ai*, *oi*. Long dorsal *v* may occur in ‘stIrring, pUrring,’ but the short vowel *ə* seems to be much more common in such cases.

Monosyllabic vowel triphthongs, as *aiə̯*, *auə̯*, etc., seem to sometimes be used; they are not easy to distinguish from the dissyllables *ai-ə̯*, *au-ə̯*, etc. Theoretically ‘flowers’ has two syllables and ‘flours’ one; in ordinary pronunciation they are identical.

Many persons do not use the point-modified vowels at all, but employ the dorsal ones instead, so that ‘arms’ = ‘alms,’ ‘leader’ = ‘Leda,’ etc., as in southern England. A serious fault of the alphabet used in Le Maître Phonétique, and of that of Dialect Notes, is the failure to provide any suitable signs for these *r*-modified sounds.

In a similar way could be constructed textual notations for French and for German. In the former the chief difficulty would be the *a* of ‘patte’ *pat*, ‘part’ *pa*ˈ*r*, as this italic form of a is made for only a few styles of type; it might be replaced by roman a or A. Such notations as *an*, *aŋ*, etc.,[2] should not be used for the nasal vowels, since they encourage the foreigner's prejudice against learning or using the proper pronunciation; the signs ***a ɛ o ɜ*** could be used for these sounds. The French sound-system, as reckoned by PASSY,[3] would then be written *b p m v f w d t n*

[1] Stressed “long u” following any lingual consonant other than *k* or *g*, in the same word, has no *j* in most parts of the United States.

[2] SOAMES, Introduction to Phonetics, 2d edition, London 1899.

SWEET, Primer of Phonetics, Oxford 1890.

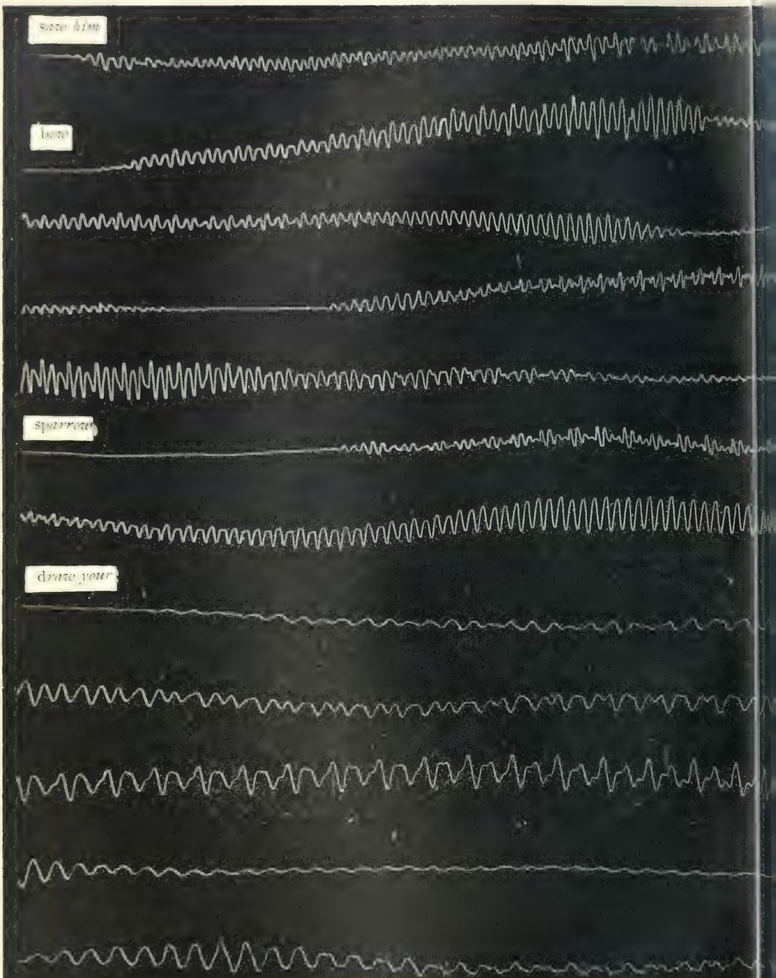
[3] PASSY, as before, 133.

z s ʒ ʃ l r ɲ ʎ ŋ g k (h) i e ɛ a ɑ ɔ o u ɜ ø y ɒ **ɛ ɑ o ɜ**, each of the vowels except *ɒ* being capable of lengthening without difference of quality. German may be transcribed with the signs *b p m v f d t n z s (ʒ) ʃ l r j ç g k ŋ (γ) χ ; h iˑ i eˑ ɛ ɛˑ aˑ a ai au ɔ ɔy* (or *ɔi*, or *ɜi*) *oˑ u uˑ ɜ øˑ y yˑ ə*; if the quality of short i, u, ü needs to be distinguished, *ɩ ɷ ʏ* could be used for them.

The use of the stress-marks can be much simplified in texts where the nomic word-division is retained.

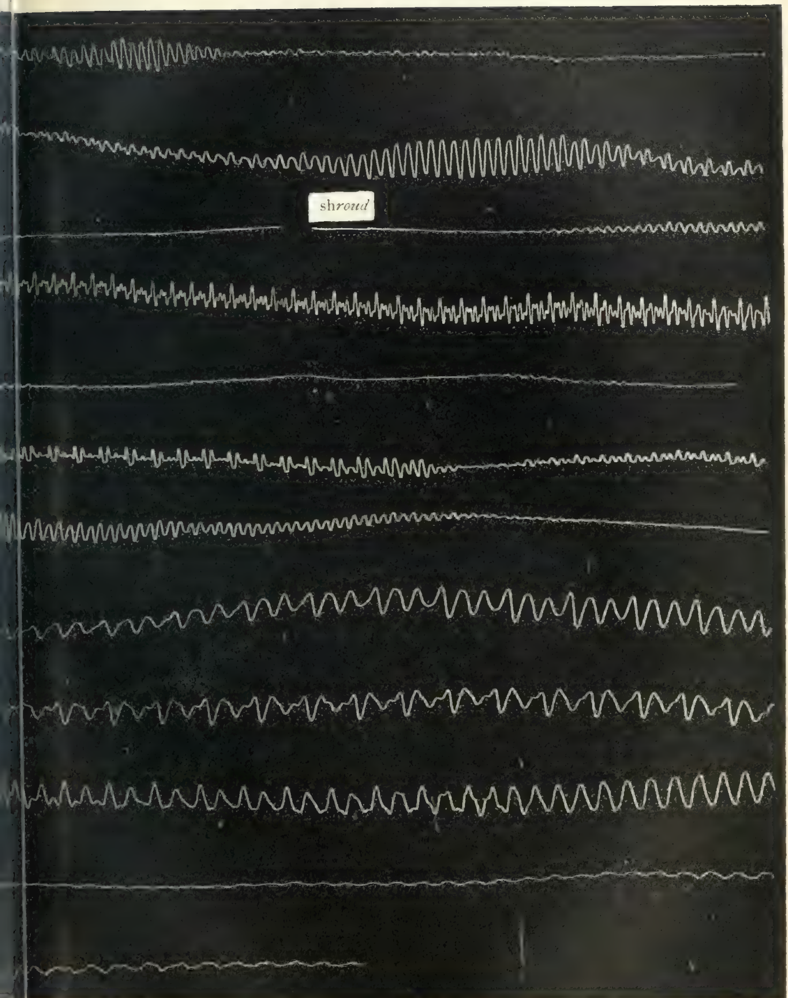
Weak-stressed syllables do not generally need to be marked as such, except in the case of monosyllables.

A large number of polysyllables in Germanic languages have the first syllable stressed ; in the Romance languages, except French, the penult is commonly accented ; the · can therefore be omitted before these syllables.

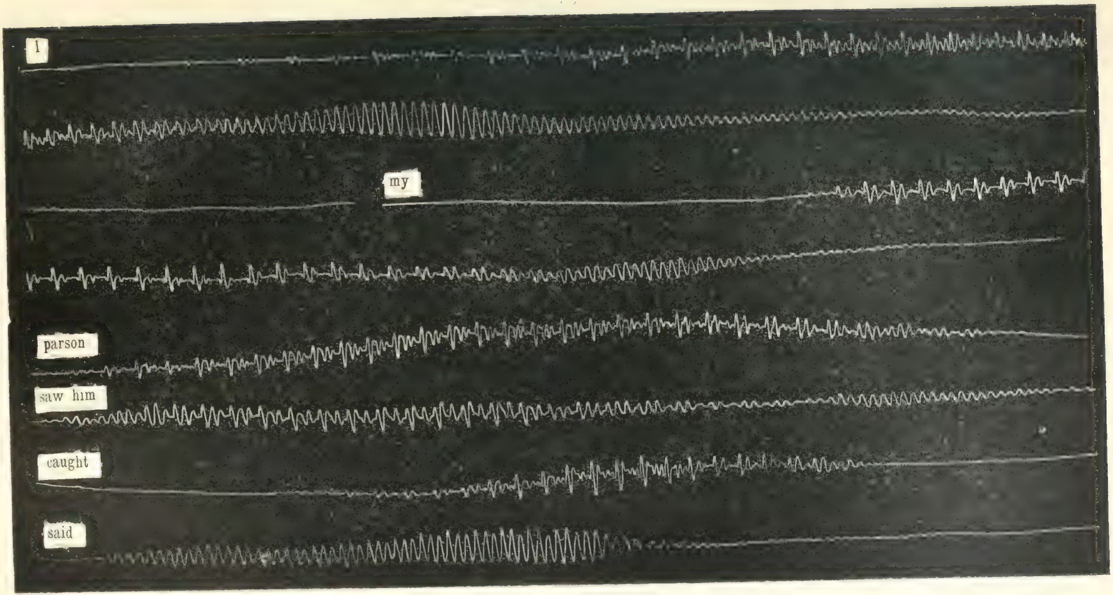


(First seven lines, $1^{mm} = 0.0016^{s}$; last five lines, $1^{mm} = 0.0007^{s}$.)

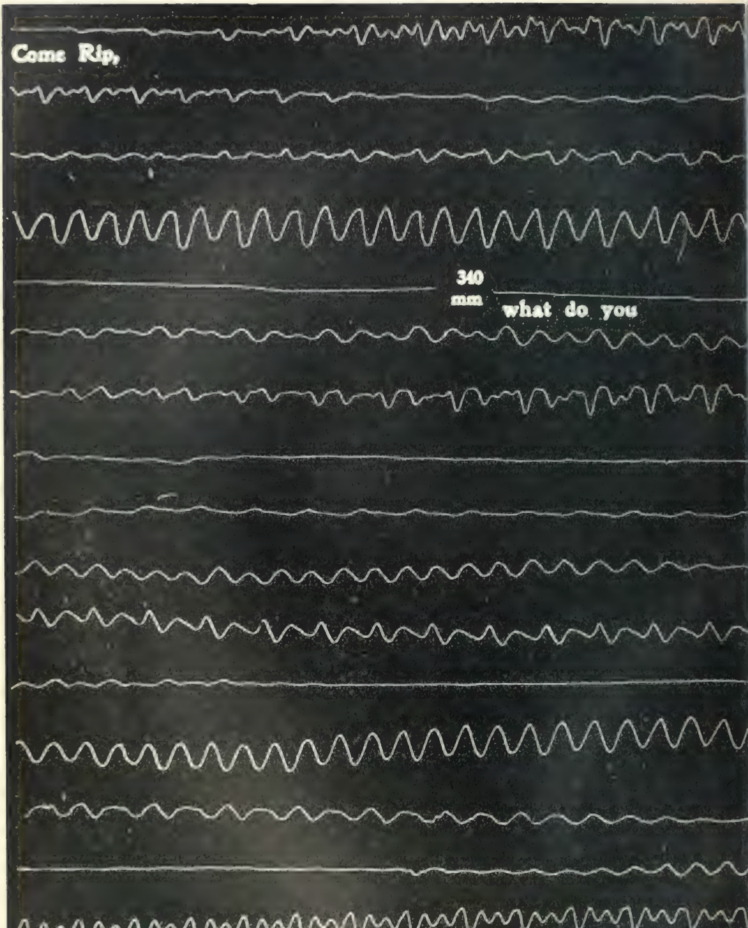
PL

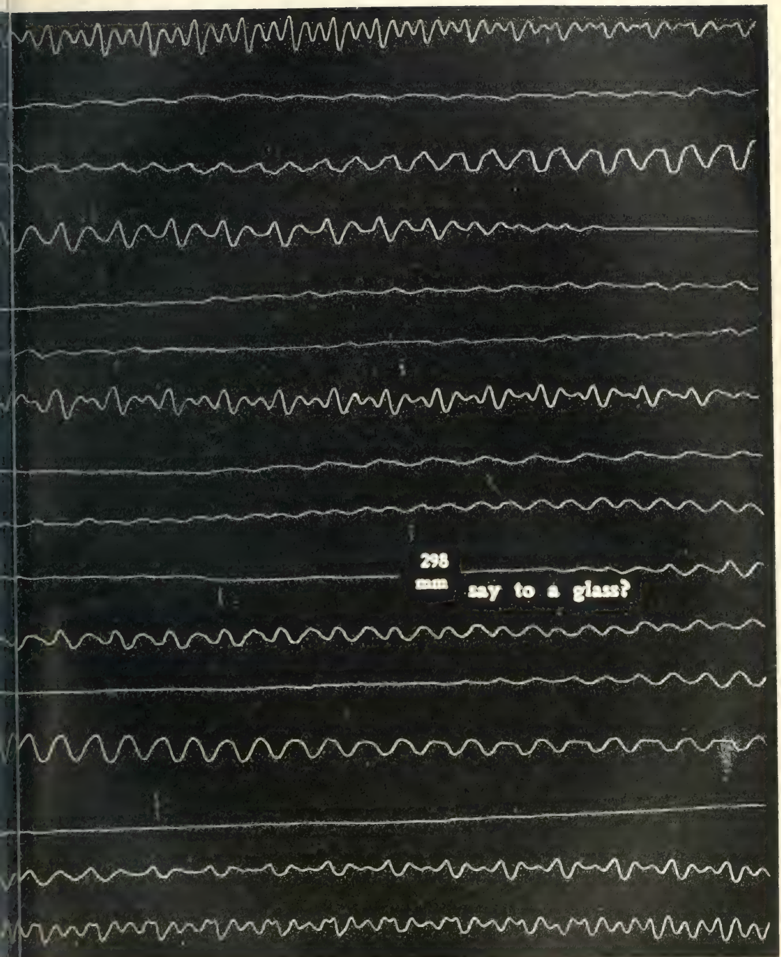


E I. Curves from *Cock Robin*, *Series II.*

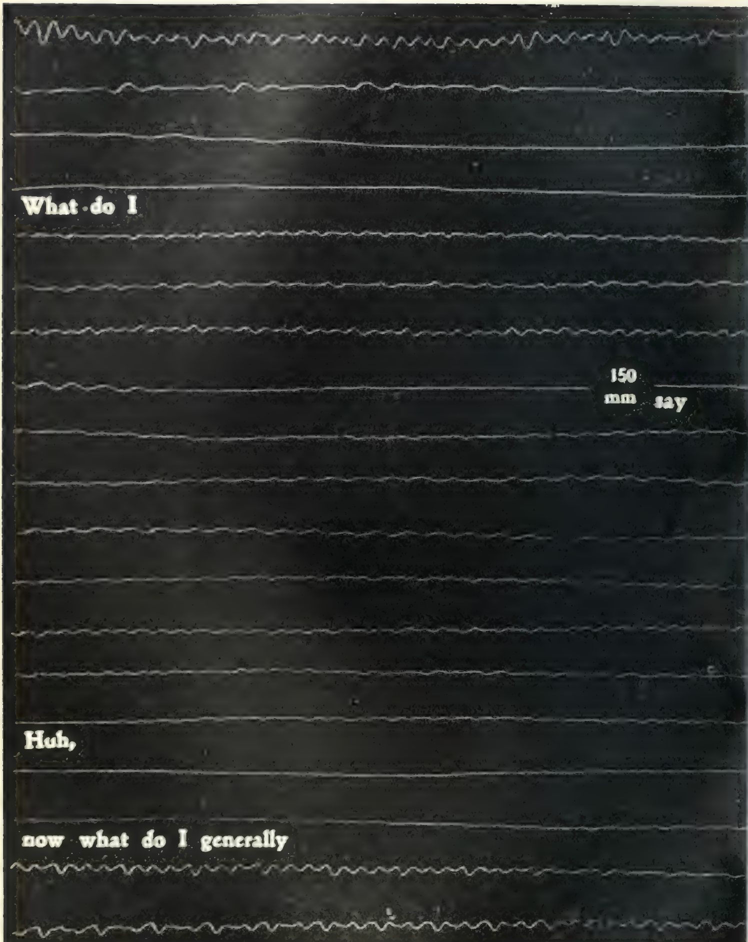


($1^{mm} = 0.0016^{s}$.) PLATE II. Curves from *Cock Robin*, *Series II.*


Come Rip,
340 mm
what do you

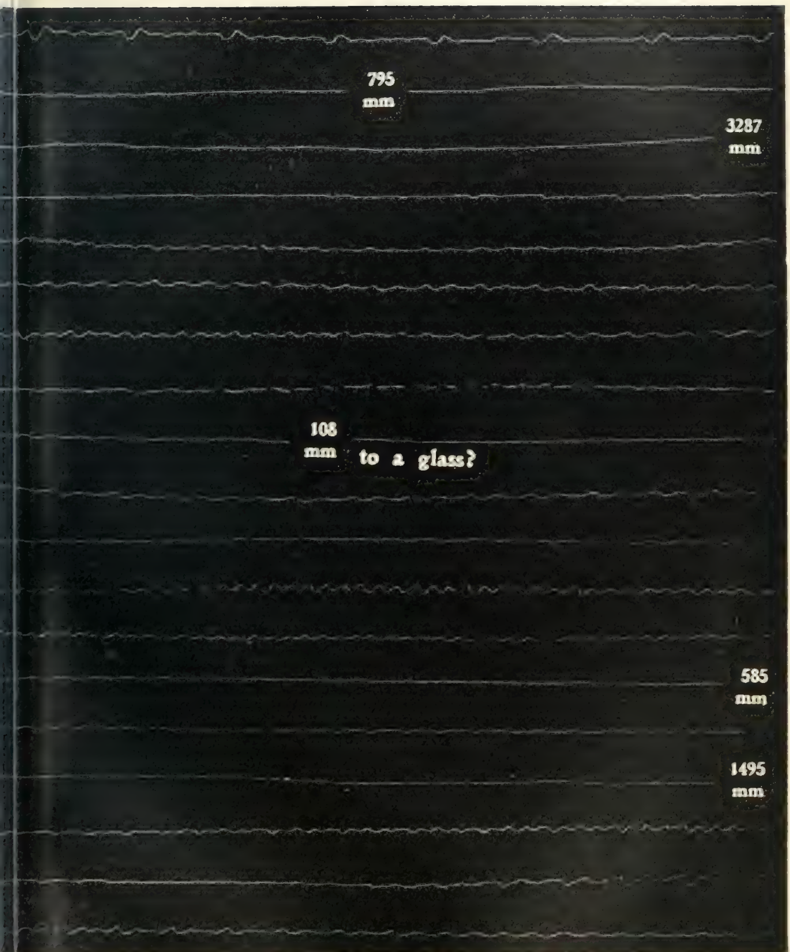


E III. *Rip Van Winkle's Toast*, by Joseph Jefferson. (Bloc .)

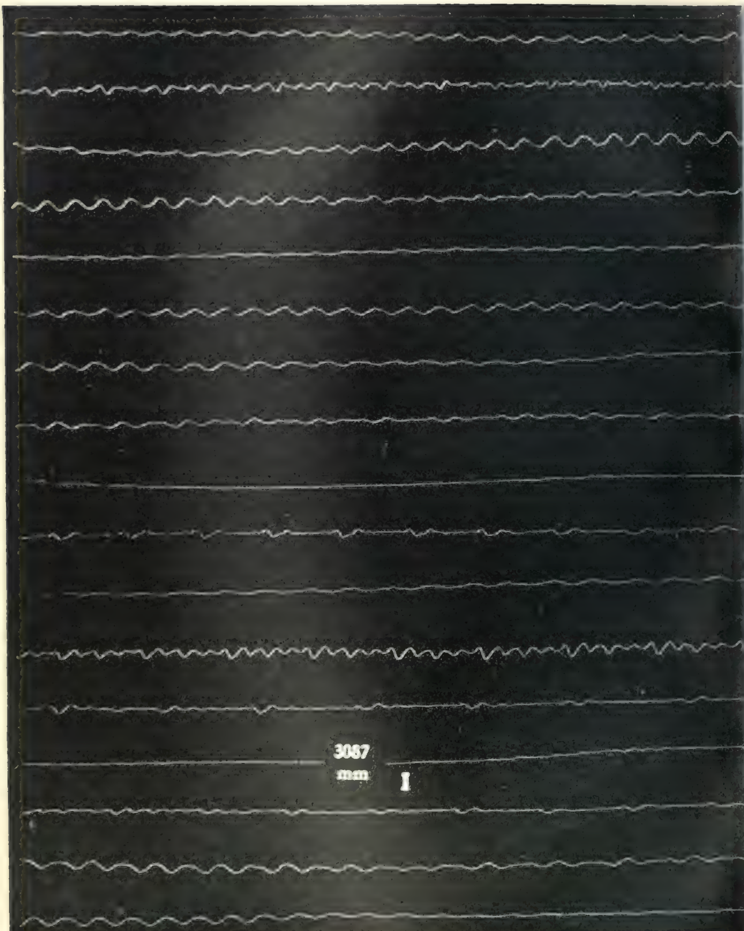


($1^{mm} = 0.0007''$.)

PL



(PLAT)E IV. *Rip Van Winkle's Toast*, by Joseph JEFFERSON. (Block II.)



($1^{mm} = 0.0007^{s}$.)

PLATE

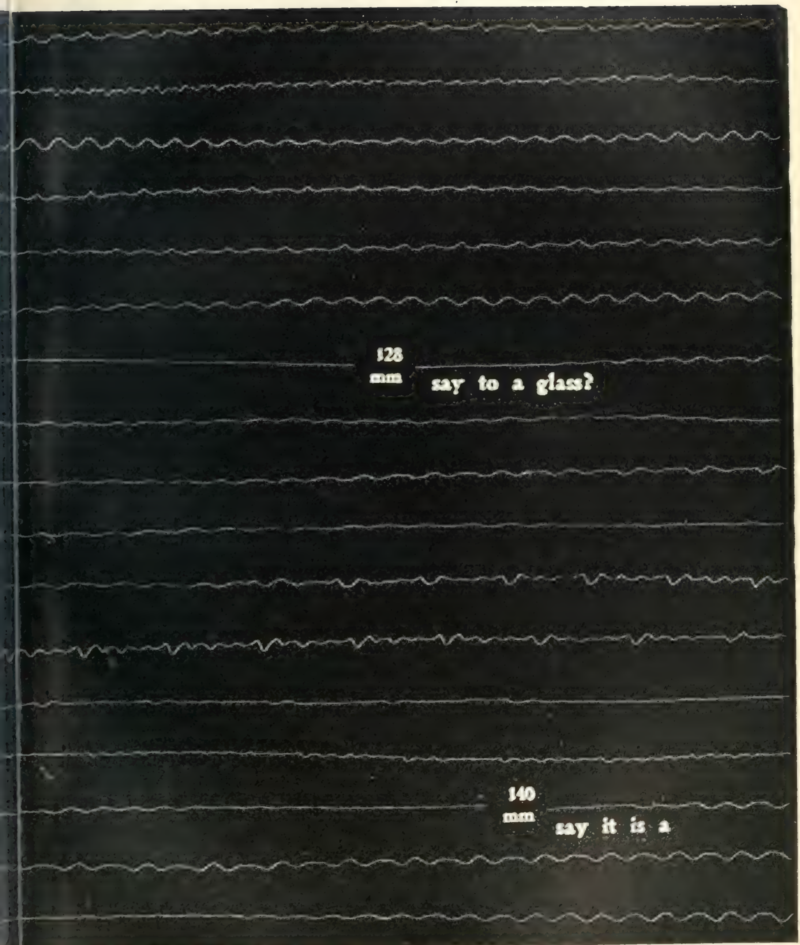
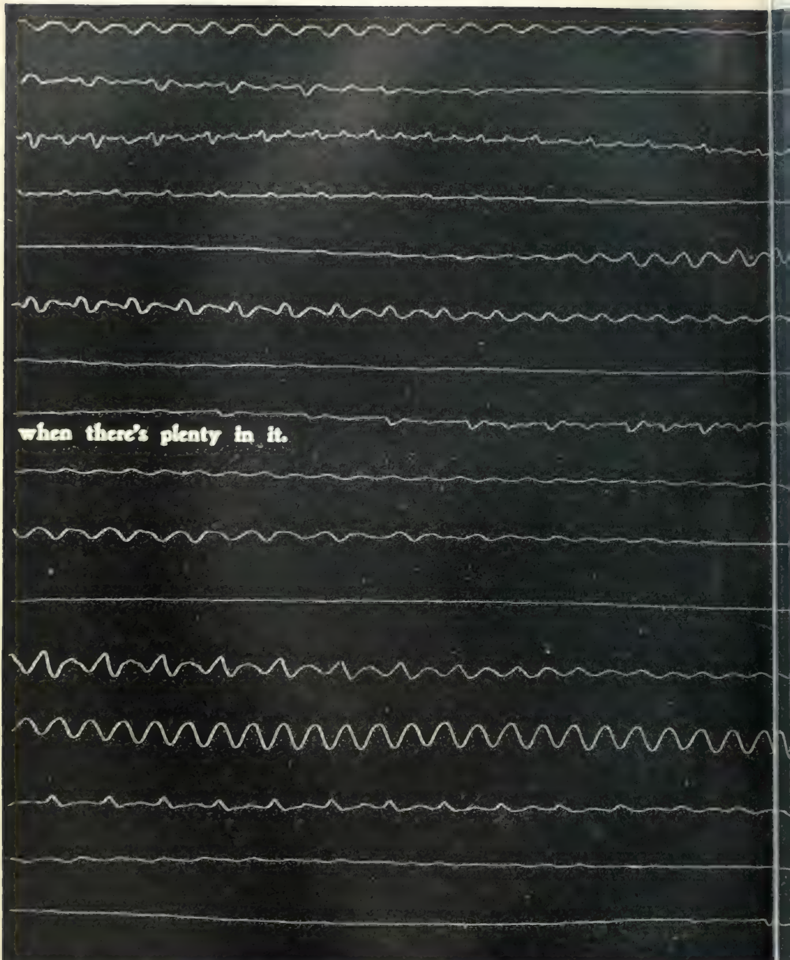
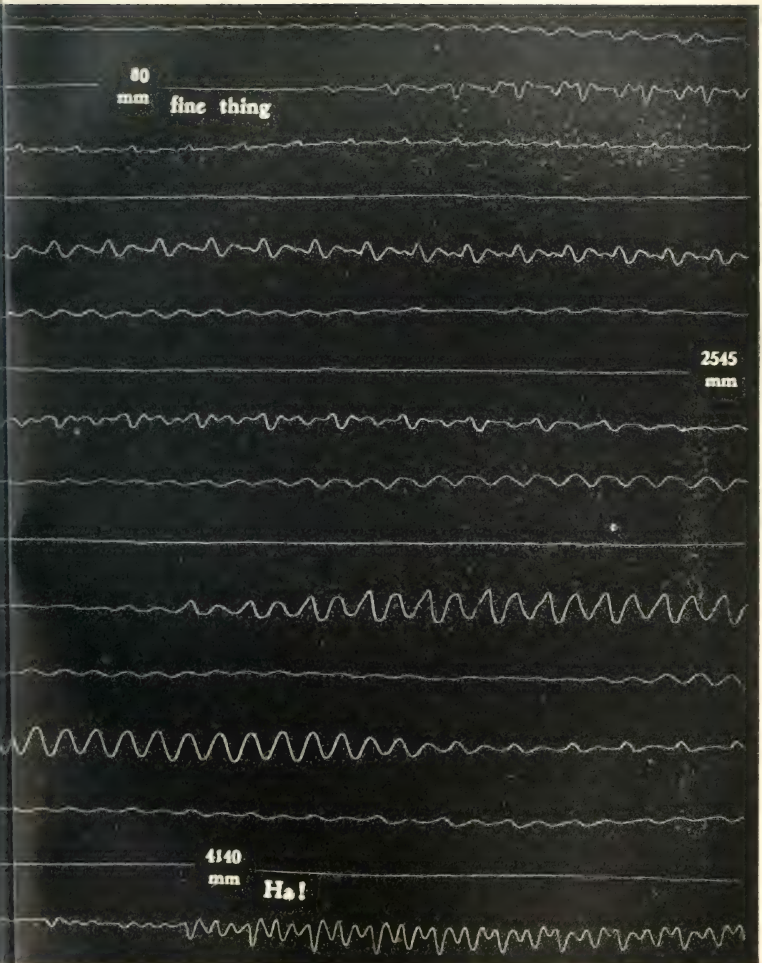


PLATE V. *Rip Van Winkle's Toast*, by Joseph JEFFERSON. (Block III.)

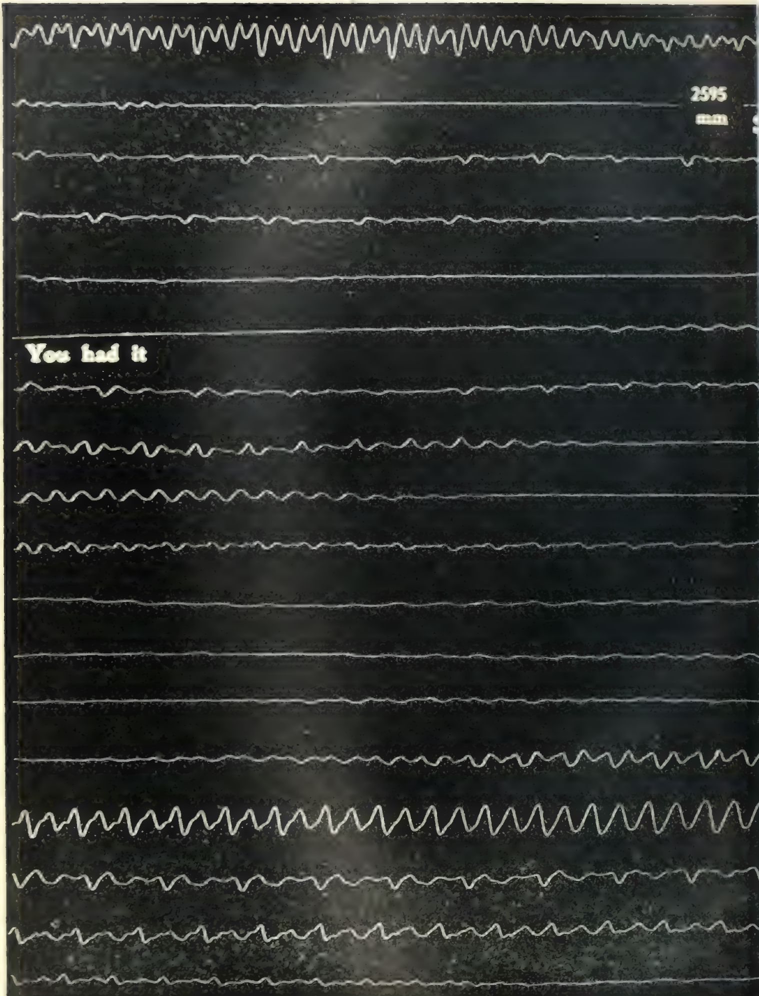


(1mm = 0.0007

PLAT

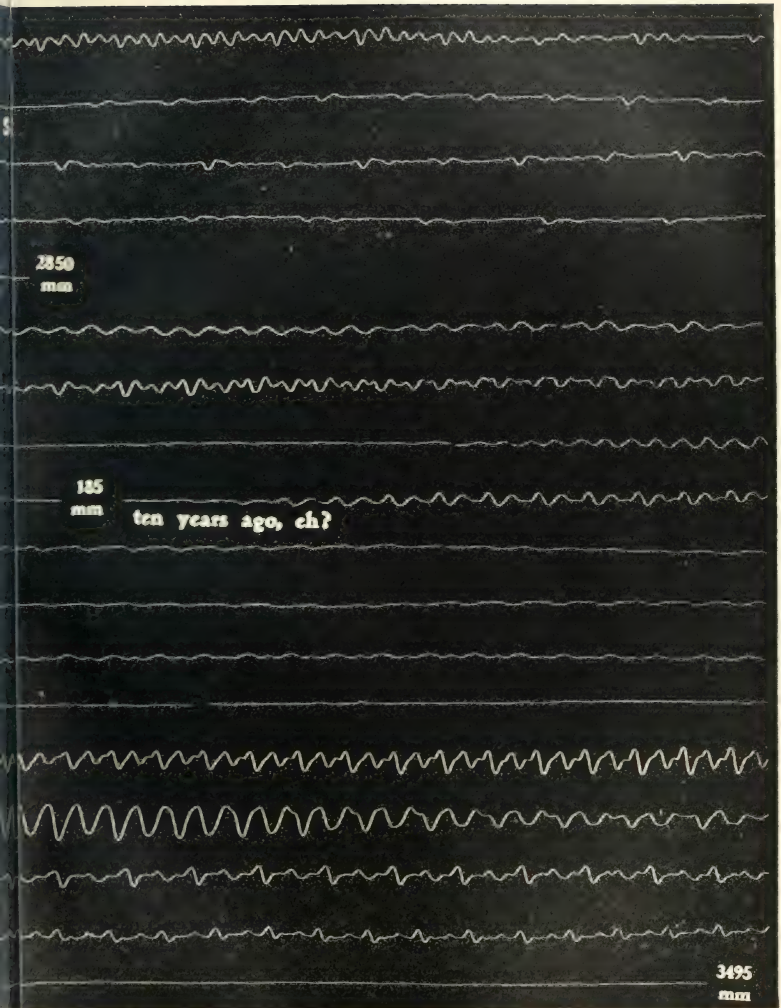


VI. *Rip Van Winkle's Toast*, by Joseph JEFFERSON. (Block IV.)

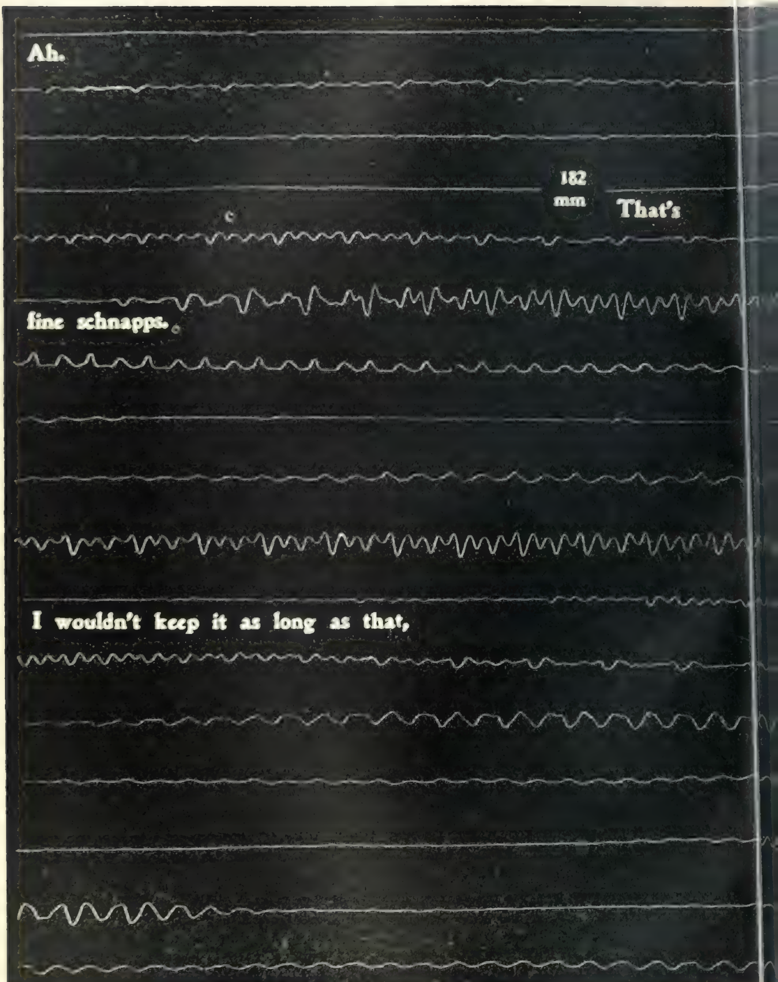


($1^{mm} = 0.0007^{s}$.)

PLAT

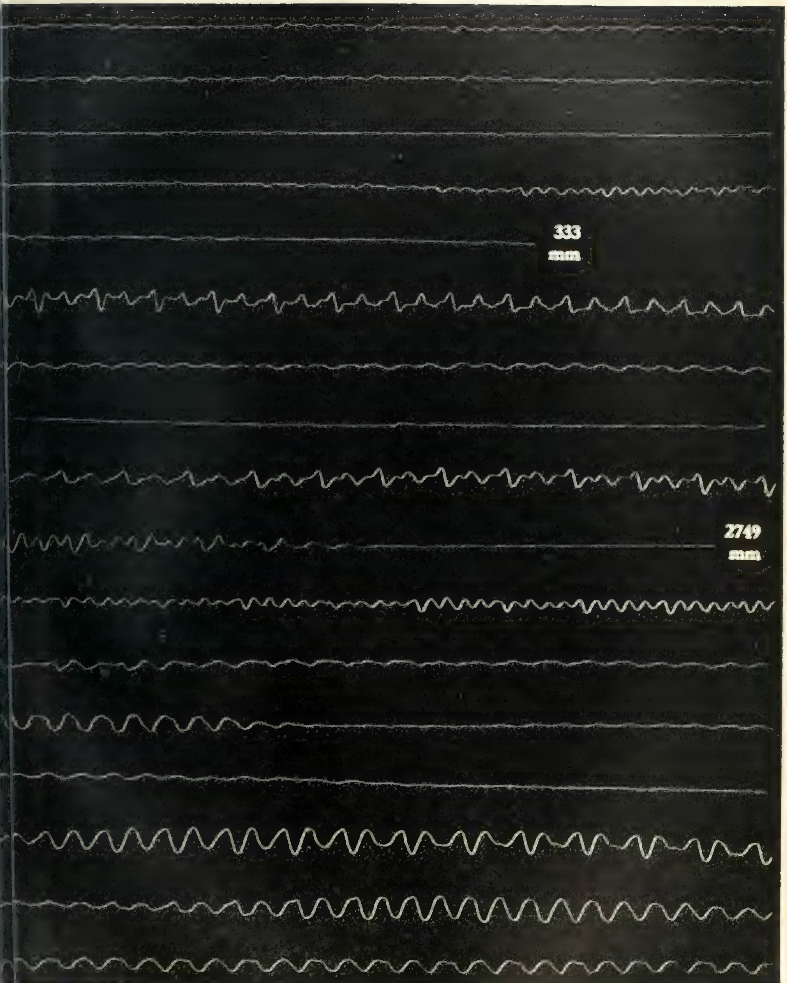


VII. *Rip Van Winkle's Toast*, by Joseph Jefferson. (Block V.)

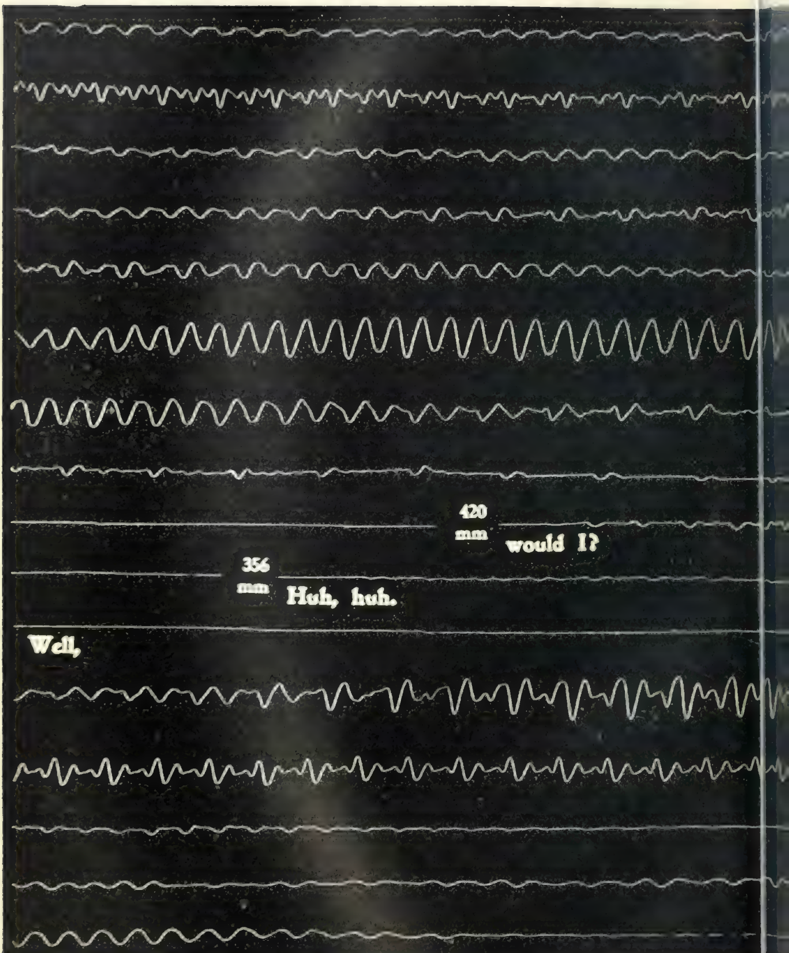


(1^{mm} 0.0007^{s}.)

PLA

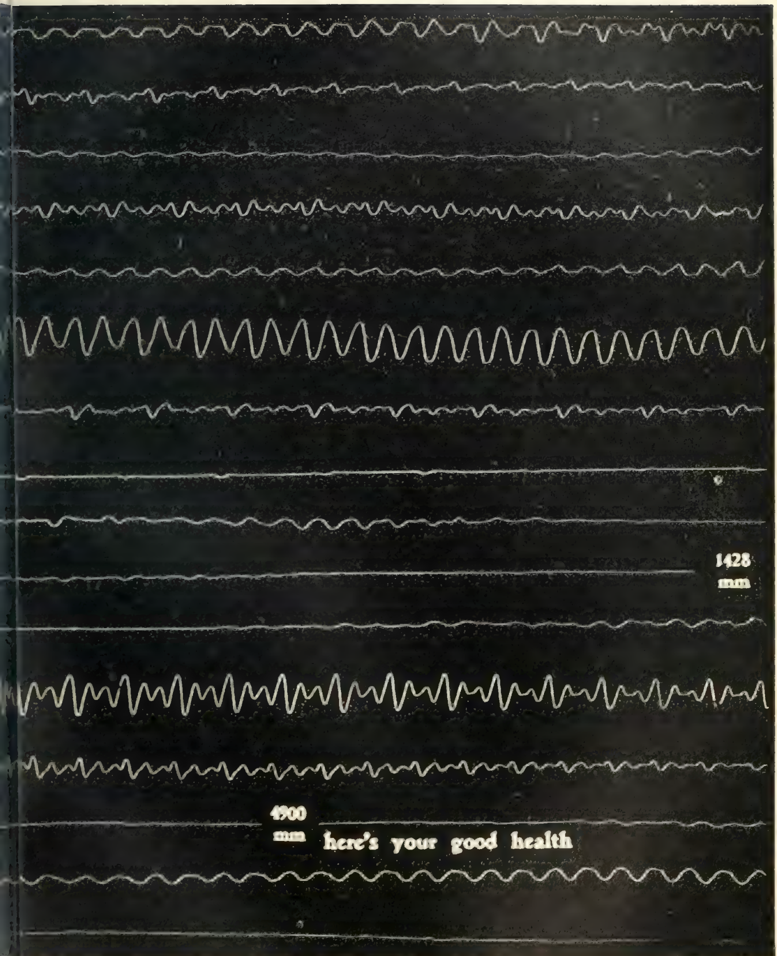


VIII. *Rip Van Winkle's Toast*, by Joseph JEFFERSON. (Block VI.)

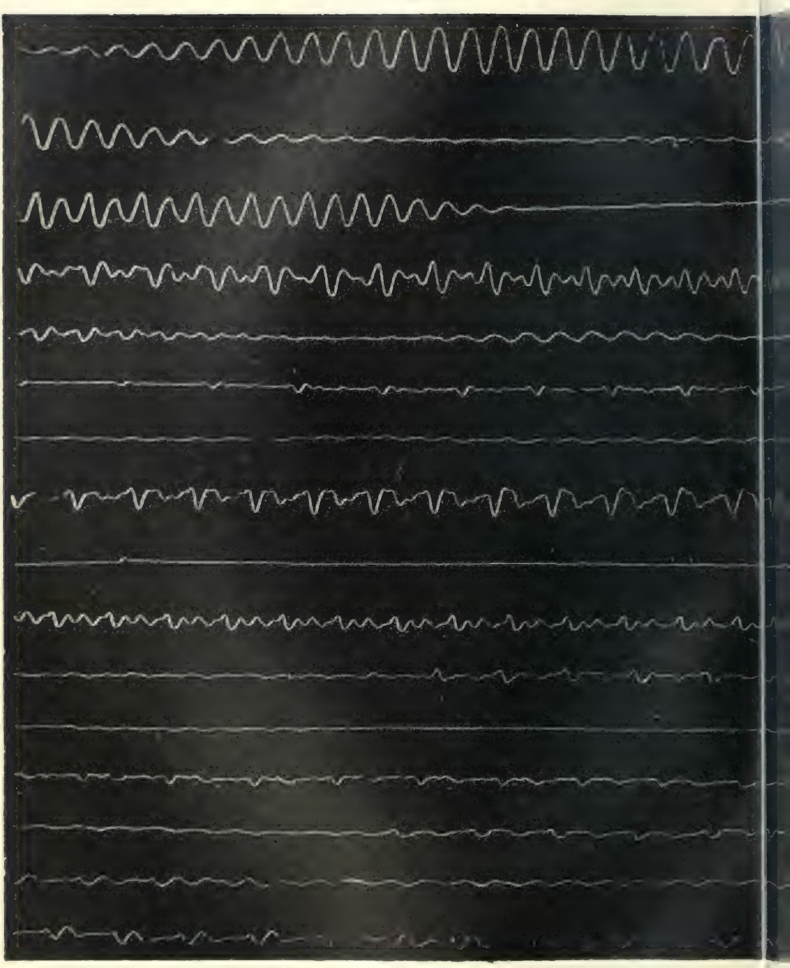


($1^{mm} = 0.0007^{s}$.)

PLA



[PLATE] IX. *Rip Van Winkle's Toast*, by Joseph Jefferson. (Block VII.)



($1^{mm} = 0.0007^{s}$.)

PLAE

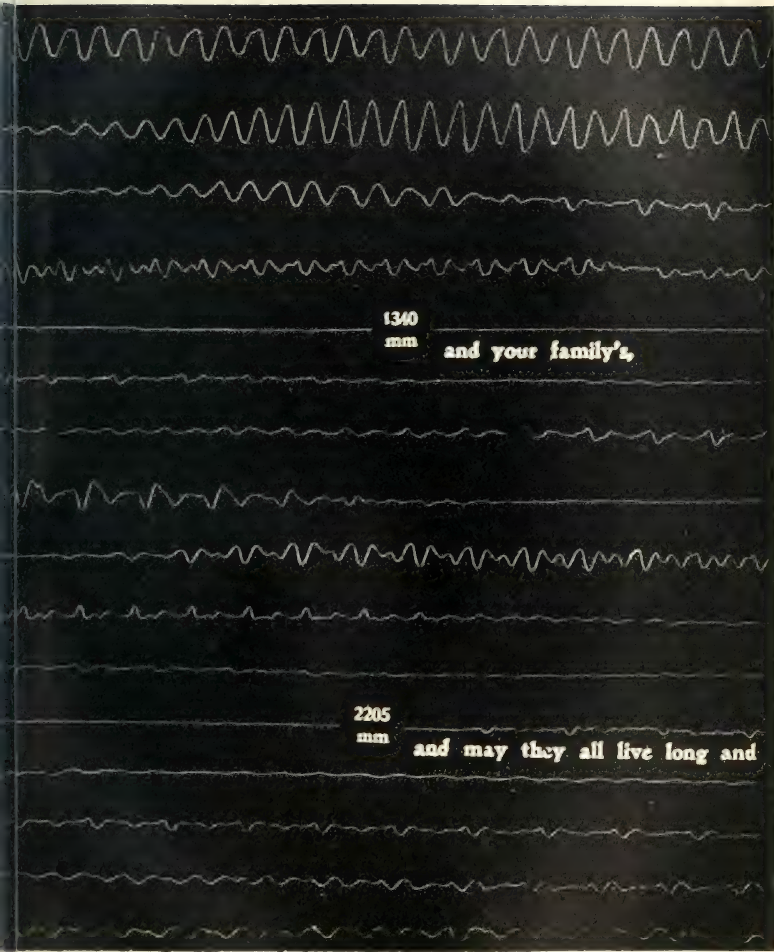
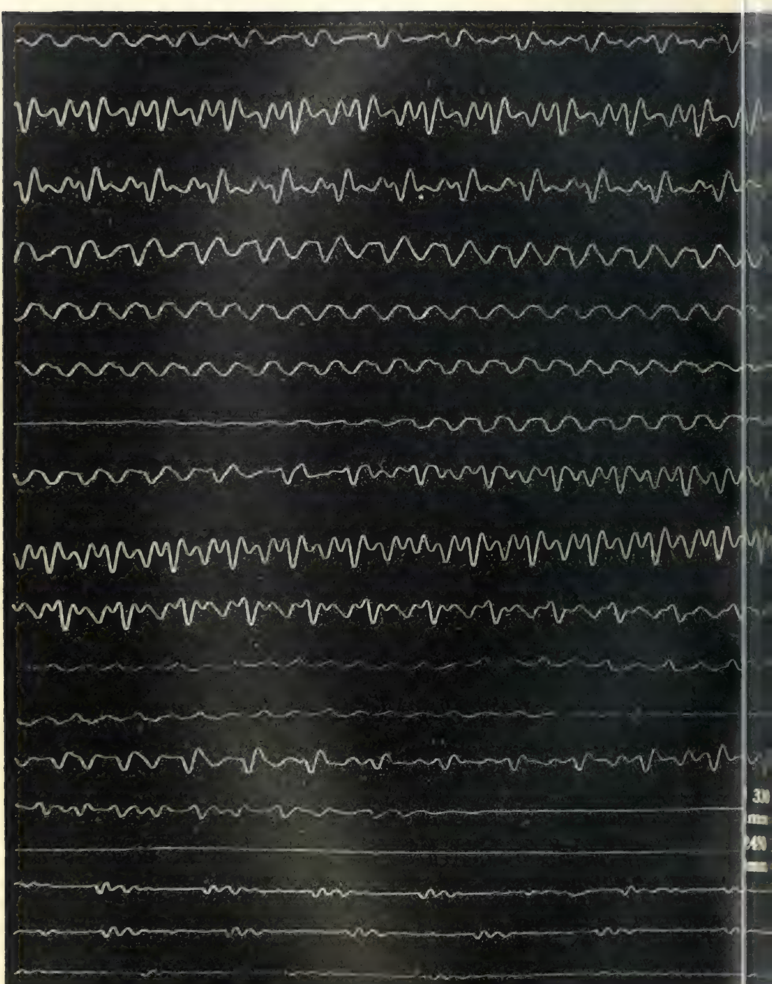


PLATE X. *Rip Van Winkle's Toast*, by JOSEPH JEFFERSON. (Block VIII.)



(1mm = 0.0007s.)

PLATE

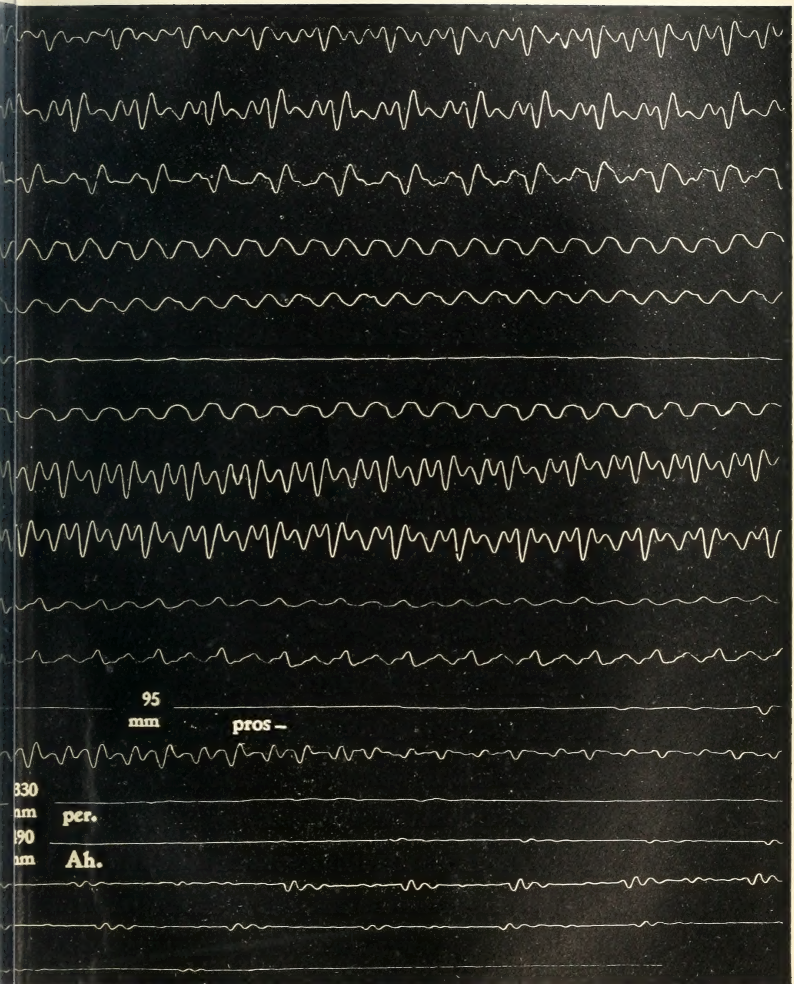


PLATE XI. *Rip Van Winkle's Toast,* by Joseph Jefferson. (Block IX.)